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LECTURES BY M. DE BLAINVILLE ON COMPARATIVE OSTEOLOGY.

THE

COMPARATIVE OSTEOGRAPHY

OF THE

SKELETON AND DENTAR SYSTEM

IN THE FIVE CLASSES OF

VERTEBRAL ANIMALS,

RECENT AND FOSSIL.

BY M. H. M. DUCROTAY DE BLAINVILLE,

Member of the Institute of France.

WITH GRAPHIC DELINEATIONS BY M. WERNER;

EDITED (FROM THE FRENCH) AND

ADDITIONALLY ILLUSTRATED

WITH NUMEROUS NOTES, OBSERVATIONS,

AND DRAWINGS,

BY ROBERT KNOX, M.D.,

Edinburgh; Corresponding Member of the French Academy of Medicine, &c. &c.

DESCENDING SCALE.

ALL the Simiæ, or African monkeys, which continue the descending series, from the P. Sabæus, or typical specimen, and, indeed, from the most inferior of the Guenons even to the lowest of the Baboons, present, in the number of portions of the skeleton and its general form, a resemblance almost complete, with the exception, however, of the tail, which, in our typical Callitriche (Fig. 2, page 298, LANCET), very long, diminishes gradually to nothing, even before we have reached the species placed lowest in the scale; but the proportional shortness of the trunk, the compression of the thorax, the shortening of the limbs, and especially of the posterior limbs, whilst at the same time they become stronger, the proportional length of the metatarsal bones, compared with those of the toes, the distal phalanx, or nail-bearing bone, becoming more and more narrow, that is, compressed laterally, and especially the elongation and narrowness of the whole head; the backward position of the occipital condyle, the elevation of the superciliary crests, the proportional elongation of the face, and, consequently, of the jaws, increasing as the cranium diminishes; the obliquity of the two branches of the lower jaw, and their mode of terminating at the chin;—all these, we repeat, constitute a "tout ensemble," or combination of peculiarities, which go on always increasing in proportion as we proceed in the descending scale, from the Callitriche to the last of the Guenons, and Baboons (*Papio*, *Macaque*), with or without the tail, to reach its *maximum* in the lowest of the Baboons or Cheiropitheci.

The skeleton of the Babouin Noir, Fig. 28, proves the observations which we have just made almost to demonstration, and we shall, therefore, confine ourselves to a very few general observations.

In the more perfect Macaque or Cynopithecus, whose tail is, assuredly, proportionally as long as in the Guenon or Cercopithecus, we can find no other appreciable comparative differences than that degradation which we have pointed out in a general way; we shall only remark, therefore, the greater elevation and thickness of the superciliary crests; the orbitar margin presents, towards its internal third, a notch with a hook, for the passage of the superciliary nerve, which can scarcely be said to exist in the Guenons.

In respect to the species in which the tail diminishes more and more, the number of the coccygeal vertebræ also diminish, as a matter of course; but, at the same time, they are smaller, and decrease more rapidly. In other respects, the number and proportions of the bones are similar. The number of the vertebræ is seven cervical, twelve dorsal, seven lumbar, three sacral, and from nineteen to twenty-two coccygeal.

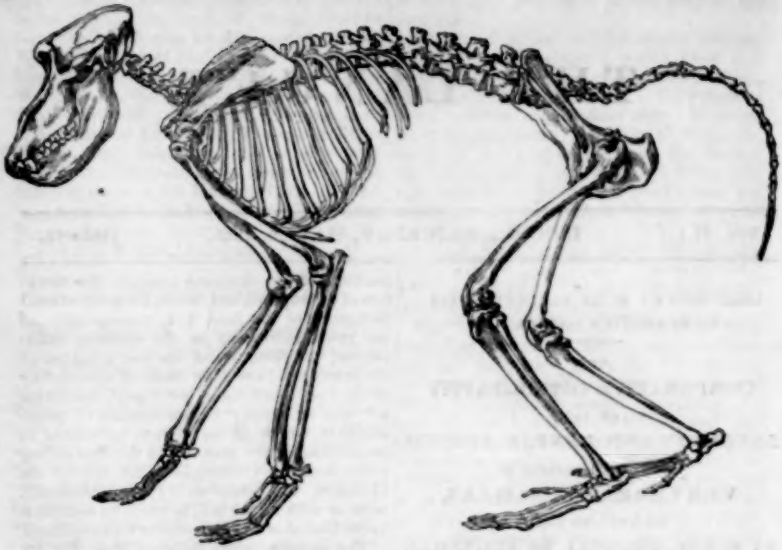
The number of the sternebræ is eight.

The ribs are twelve in number, of which eight are sternal and four asternal.

There is a ninth bone in the carpus.

The Cynopitheci without a tail, or the Magots, are similar to the Macaques, that is to say, all the characters of degradation manifest themselves more and more; but besides the tail being reduced externally to a small rudiment, or fleshy lobe, the sacrum terminates abruptly in becoming much con-

Fig. 28.



Babouin Noir (*P. Porcarius*). After a natural skeleton in the Paris collection. The scapulæ are slightly displaced, and the canine teeth are much worn.
Cynocephali Chacma, Cynocephalus, Porcarius, Desm.
Papio Comatus et Porcarius, Geoff.
Singe Noir, Vaillant.
Habitat, Cape of Good Hope.

tracted and thinner, and the coccyx is reduced to a few vertebræ, more or less deformed, very flat, provided, however, with transverse processes, though irregular in form.

The number of bones composing the skeleton is, in other respects, the same; the spinal column, consisting of seven cervical, twelve dorsal, seven lumbar,* three sacral, and two coccygeal vertebræ.

There are seven sternebræ in the sternum and twelve pairs of ribs, of which eight are sternal and four asternal.

There are nine bones in the carpus, of which the intermediate bone forms one.

In the Baboons, or Cheiropitheci, which terminate the series of the Simiæ, or African Monkeys, and in which the degradation, in all the peculiarities noted above, reaches the highest degree of development, as one may at any time satisfy himself by a glance at the skeleton of the Babouin Noir, of the Cape of Good Hope (see Fig. 28); the number of bones remain, with certain exceptions, the same as in the Simiæ higher in the scale; but at bottom the difference falls really on the additional asternal rib, which causes one less of the lumbar vertebræ. In fact, the total number of the vertebræ, with the exception of the coccygeal, is always twenty-nine, viz., seven cervical, thirteen dorsal, six lumbar, and three sacral.

The number of the sternebræ, also, is not changed; there are generally seven or eight, the one anterior to the last being rudimentary, although the increased number of ribs should be only on the asternal, which are five in place of four, the sternal ribs being eight, as in all the Simiæ with the narrow sternum. The form of the ribs is, in general, smaller and more straight than in the preceding species.

But, on the subject of the number of ribs, and, consequently, that of the dorsal and lumbar vertebræ of the Baboons, I may observe, that, assuredly, the more ordinary number of the ribs is thirteen pairs, thirteen dorsal vertebræ, and six lumbar, but to this we may meet with exceptions. Camper noticed one exception in a male Cynocephalus which had only twelve pair of ribs, and, consequently, twelve dorsal vertebræ; but there were, also, only six lumbar vertebræ. The osteological collection of the museum in

* Camper found only six lumbar vertebræ in the Magot, which he examined; there were twelve dorsal, and, of course, twelve pairs of ribs.

Paris, shows a similar exception in the male *Pithecus Sphinx*, in a male *P. Cynocephalus*, in three young specimens of the *P. Maimon*, of which one belonged to Daubenton, but whether male or female is unknown.

We possess, also, a female specimen of the *P. Cynocephalus*, or common Baboon, which has only twelve pairs of ribs, but in which there are eight lumbar vertebræ. In this case, then, there is really an additional vertebra. Finally, I ought here to remark, that if in the other species of Simiæ with the narrow sternum, the number of vertebræ, belonging to the trunk, is almost uniformly twelve, carrying eight sternal ribs, and four asternal; and having seven lumbar vertebræ, I may allude, also, to some exceptions which occur frequently in the Baboons; thus, in our Museum, a specimen of the *P. Nemestrinus* (a species of Macaque, a Guenon nearly allied to the *P. Diana*), has thirteen dorsal, and six lumbar vertebræ, and a Magot has four asternal ribs on one side, and five on the other of the sesamoid bones. The Simiæ possess, in their locomotive organs, a considerable number of sesamoid bones, or small bones, whose characteristic feature is being placed in certain tendons of muscles near their attachment, and rarely in the mesial line between two sets of muscles, as is the case with the pretended ossified ligament in the moles neck.

In the anterior limbs, we have, for a long time, described three sesamoids. One, the most constant, which belongs to the tendon of the long abductor of the thumb; it is present, indeed, in all the Simiæ, and even in those with the broad sternum, placed between the navicular and trapezium.

A second, seems to me to be developed in the tendon of the anterior cubital; it is not found in all the species. It is, of course, placed between the os triquetrum and pisiform bone. Is it this sesamoid bone, which has made Mr. Owen, of London, say that the last bone (pisiform) is divided into two, in the Orang-Outang. The number of the carpal bones will amount, then, to eleven.

We still find sesamoid bones, two in each of the four fingers, in the bifurcated tendon of the flexor perforatus at its insertion into the base of the proximal phalanx.*

In the posterior limbs the sesamoids are still more numerous, than in the anterior; First, in the femoro-tibial articulation, or knee joint, we find, anteriorly, the largest of these sesamoids, known under the name of the *rotula*, inclosed within the tendon of the triceps extensor cruris, near its insertion into the tibia. It is a thick bone, large, and of an oblique quadrilateral form in the Orang-Outang, in which animal it approaches the human type, being only more regularly oval, and narrower than in the inferior Simiæ.

Posterior to the knee-joint, there may be found in the Simiæ, three thick sesamoid bones; two, one in each head of the gastrocnemius muscle near its insertion into the femur, and the third, in the tendon of the popliteus muscle, also at its insertion into the femur.

The two first appear only present in the higher organised Simiæ.

In the foot we find a sesamoid in the long peroneal tendon, at its passage over the external margin of the cuboid bone; a second in the tendon of the tibialis posticus muscle, but this appears to be less constant; lastly, other sesamoid bones, in the tendinous attachment of the short flexors of the toes near the proximal phalanges. Camper speaks of another of these small bones, which he has found in the Mandrill, inclosed in the ligament which unites the calcaneum to the metatarsal bones; I have not had an opportunity of observing it.

These different sesamoid bones have, undoubtedly, a form sufficiently peculiar to enable one, in a certain degree, to distinguish the one from the other, at least in the same individual, but it would not be easy to go farther; and excepting their size, from which it is possible to trace some satisfactory characters, we cannot risque our opinion, without being almost, to a certainty, wrong. In truth, developed in the centre of tendons, which commence or terminate the muscles, and whose action they must increase and modify, we can easily perceive, that the power with which that muscular action is sustained, must have an influence on the development of the sesamoid bones, and, consequently, on their form. It is on this account that we have thought it unnecessary to give any separate figures of these bones, with the exception of the rotula (patella), and still less have we thought it worth while to describe them comparatively.

* This slight inaccuracy has, perhaps, escaped, M. De Blainville here. The tendon of the flexor perforatus must, we should think, proceed to the *middle* phalanx in the Baboon, as well as in man, and, in respect to the sesamoid bones, which are really found at the base of the proximal phalanges, and on the distal end of the metacarpal bones in man, they have appeared to us to be developed in the transverse metacarpo-phalangeal ligament. Although the position of these sesamoid bones has appeared to us, such as we have stated, yet it is not meant to deny their probable connection with the tendon of the flexor perforatus, and tendon of the interosseal muscles, and through these means, the probable connection of the same tendon with the proximal phalanx; although, unquestionably, the termination of the tendon of that muscle is in the middle phalanx, whilst the sesamoid bones are immediately connected with, and, in fact, play an important part in, the metacarpo-phalangeal articulation.—*R. Knox.*

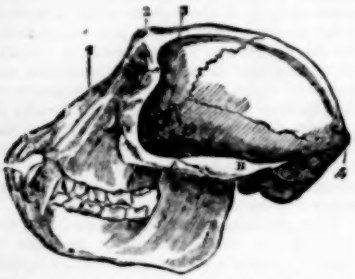
OF THE TEETH.

The dentar system of the Simiæ has, perhaps, a still greater number of points of resemblance with that of man, than we have had occasion to point out in the bones, which compose their skeleton; in a word, the number, as a whole, and of each sort of teeth, is positively the same, that is to say, there are always two pair of incisive teeth, both in the upper and in the lower jaw, a pair of canine teeth, also, in each jaw; and, lastly, five pairs of molars in each jaw, composed of two *anterior* molars, one *principal*, and two *posterior*.

The position and general form of each of these teeth corresponds, in a great variety of points, to the human structure, although, when examined with care (scientifically), we may observe very evident differences.

Following out our original plan, we shall select the Callitriche as a point of comparison.

Fig. 29.

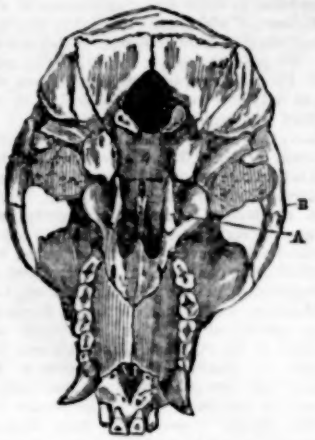


Head, viewed in profile, of the *Guenon Callitriche Cercopithecus Sabæus*, Desm. *Singe vert*, Briss.—*Simia Sabæus*, Linn. *Callitriche*, Buff.—Audel, &c.

Fig. 30.

Same, viewed from above.

Fig. 31.



Same, viewed from below.

Fig. 32.

Lower jaw, viewed from above.

The series of teeth in this Simia, in the adult state, is not perfectly continuous, the canines, for instance, are much larger than the other teeth, at first considerably exceeding the level, but afterwards, in order to cross the upper canine tooth, those in the lower jaw displace, if we may so say, the upper canine from the external incisors, thus producing a considerable interval between these two teeth.

Of the two pairs of upper incisives, the middle pair is by much the largest. The inferior incisives, equally terminal, but more parabolic in their form, are more of a size than those of the upper jaw.

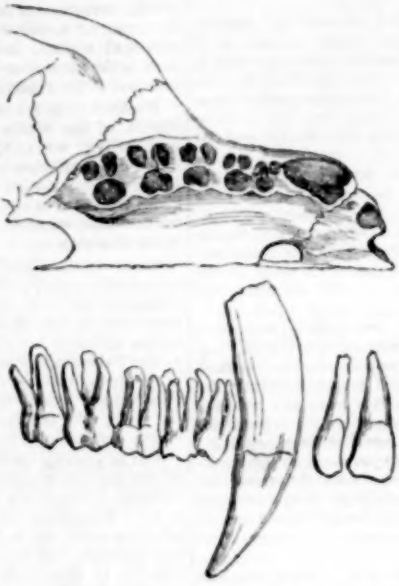
The superior canines, much longer than the inferior, are also much arched and compressed, slightly cutting and pointed posteriorly, they point slightly outwards.

The inferior canines, a little shorter, more

conical, are also slightly curved posteriorly, and point outwards.

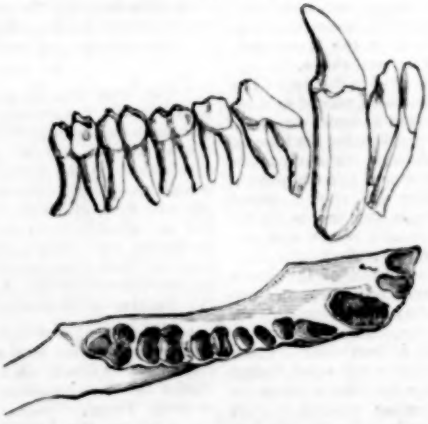
The superior *anterior* molars have their corona transversely parallelogramic, armed with two pyramidal tubercles, trihedral in the same direction, and their roots, formed of two pivots, almost similar externally, and of only one strong root internally.

Fig. 33.



Upper jaw, right side; dentar aspect, with the teeth removed, in order to show the alveolar cavities and fangs of the teeth.

Fig. 34.



Lower jaw, right side; dentar aspect, with the teeth removed, in order to show the alveolar cavities and fangs of the teeth.

The inferior *anterior* molars are a little stronger than the superior; the first, more carnivorous (cutting), more compressed, more oblique; the second, with a heel more developed, but both with two simple conical roots, that of the first very strong, particularly its anterior root.

The *principal* molar, as well the superior as inferior, becoming more square, as if composed of two smaller anterior molars, gives to the corona four tubercles in two transverse bicuspid elevations; with three roots in those of the upper jaw, four in those of the lower, united into two similar and flat pivots.

The two superior *posterior* molars are larger than the *principal* molar, but they have the same form, with the exception, perhaps, of the last, which is a little smaller, its anterior half being larger than its posterior.

The roots are also similar to those of the principal molar, only a little stronger and more diverging.

In the lower jaw, the two *posterior* molars are similar to those in the upper jaw, that is, they are larger than the *principal* molar; but we shall presently find that this is not uniformly the case, the last molar often presents a remarkable difference, in having a fifth or posterior tubercle. The roots are like the preceding.

After what we have just said on the correspondence of the corona with the roots, we observe how the knowledge of the anatomy of the alveoli comes to be of importance in all scientific researches, in comparative anatomy.

The alveoli for the incisive teeth are, like the teeth themselves, always simple, larger in the upper jaw, particularly for the first pair, and also less compressed than in the lower jaw.

The alveoli for the canine teeth are equally simple, but are much larger, still more compressed, and subtriangular, more so, however, in the upper than in the lower jaw, where they are of a more oval shape.

The alveoli of the molars in the upper jaw are all formed by three holes, placed in a triangular form, two, on the external margin of the jaw, and one, larger, forming the internal angle. The only difference, for each tooth, consists in the greater extent and diameter of the holes, being closer and more serrated for the *anterior* molars, a little less so for the *principal* molar, and nearly of the same dimensions, for the *posterior* molars.

With regard to the alveoli of the lower jaw (see Fig. 34), they are altogether differently arranged; they are composed, for each tooth, of two holes, transverse, more or less equal, and more or less close. Those for the first *anterior* molar are more distant and more unequal than the others; those for the second *anterior* molar, nearest to each other, which, moreover, have the anterior hole shaped like a key-hole; it is also the largest of the two, the posterior being oval, more open, and quite of a different form. The same remarks apply to the alveoli for the *principal* and two *posterior* molars.

Moreover, considered as a whole, the alveolar edges constitute a parabolical figure, with longish branches slightly curved inwards, nevertheless more contracted towards its posterior termination, than opposite the principal molars, and this may be readily seen in the view from below (Fig. 31) of the cranium of the typical Simia.

We have already remarked, that the dentar system of the Simiæ presents a remarkable uniformity; we can, however, point out differences, sufficiently important to be mentioned; but I do not think that these differences would so evidently follow a series, as those which we have already particularised in the skeleton.

What we have just written is taken from the species which we have selected as a type of comparison, viz., the *P. Sabæus*, and the same descriptions apply, with little modification, although we ascend the scale as far as the Talapoin, the most perfectly developed of the genus *Cercopithecus*, or descend to the Dos-Rouge,* which is placed at the bottom of the scale, with the exception, perhaps, of the length and strength in the canine teeth.

But in passing to the Semnopitheci, with or without the thumb, which constitute the Colobes of Illiger,† in the ascending scale, or to the Cynopitheci or Macaques in the descending scale, we find a peculiarity in the last posterior molar in the lower jaw, which consists in that tooth being provided with a fifth tubercle posterior to the other four tubercles, which gives it a greater length than the others, and, consequently, a root posteriorly, and an alveolar hole, rather differently placed.

In other respects, the dentar system of the Semnopitheci is altogether similar to that of the other Simiæ, and such as we have de-

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* The term Dos-Rouge, employed by M. De Blainville, is too vague to enable us to determine precisely to which particular species he alludes, but we presume it to have a reference to the lowest of the Guenons, which, according to Desmarest, is the Cercopithecus Atys, although this is evidently not the one alluded to by our author. Illiger, Lacepede, and others, continued to apply the term Cercopithecus to the genus following the Guenons in the descending series, or the Macacus of Desmarest.—*R. Knox.*

† The genus Colobus is the fifth genus of Desmarest, and the term seems to have been more used by Geoff. than by Illiger. It includes the *Simia Comosa* (*full bottom Bay Monkey*, Penn., *Colobus*, Temmeck.) This genus follows the Pongo of Wurm., and, of course, greatly precedes the Guenons, according to that author.

scribed, except, perhaps, that the transverse eminences are a little more distinctly marked.

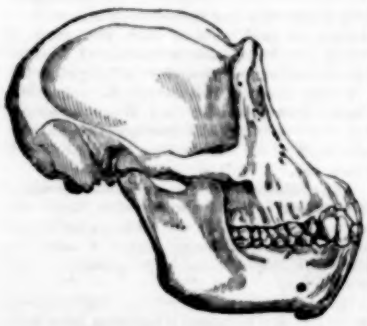
In the Soulili, whose dentar system, like the skeleton, approaches the human type, the similarity to the Gibbons may have already been remarked in the more rectilineal disposition of the incisors, in their greater regularity, in the diminished development of the canines, and, consequently, in the more regular arrangement of the first inferior *anterior* molars. In other respects, the molar teeth have their ridges almost transverse, and the last inferior *posterior* molar has not a vestige of the fifth tubercle. The *principal* molar is a little stronger than the posterior molars.

The Gibbons (figs. 17 and 22), with the large and flat sternum, have a sufficiently peculiar dentar system, not perhaps with regard to the incisives or canines, which are nearly that of the soulili, but in the *posterior* molars; in fact, in all the species of that sub-genus, the tubercles of those teeth have their transverse ridges not so regularly arranged; they are, on the contrary, very oblique, and other minute tubercles are developed on them which gives the corona a more rounded form than in the other genera of the Simiæ. This disposition does not always hold however, excepting with regard to the lower jaw, in which the three *posterior* molars present, externally, three small tubercles; internally, two; in the upper teeth, there are only four tubercles perfectly distinct.

With regard to the proportions of these three molars, it is the same in the upper as in the lower, the intermediate or the first *posterior* molar, being the largest, and the last the smallest.

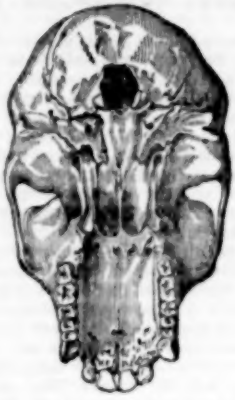
But this peculiar character is no longer found in the two great species of the Simiæ, which stand at the top of the scale, not even in the Orang-Outang, which so closely approaches the first of the Gibbons.

Fig. 35.



Head of the Chimpanzée, viewed in profile.

Fig. 36.



Head of the Chimpanzée, viewed from below.

In the Orang-Outang, as in the Chimpanzée, the incisives are largely developed, especially the first pair of the upper jaw; the canines are also very powerful, extending much beyond the dentar line, and, consequently, crossing each other considerably; but they are always conical, thick, very slightly, or not at all, bent, and have no cutting edge posteriorly.

The molars have a strong resemblance to the human, only the *anterior* molars are slightly more carnivorous (cutting) on their external margin, and the first *anterior* molar in the lower jaw, in the Orang-Outang especially, is larger, more triangular than the second; but it is the *principal* and two *posterior* molars which are still more characteristic.

In the Orang-Outang the *principal* and two *posterior* molars, are nearly equal both in the upper and lower jaw, the principal molar in the lower jaw being only a little stronger than that in the upper jaw.

In the Chimpanzée, the arrangement of the molars is extremely like the human; in these remarkable animals, the three posterior molars evidently decrease from the first or *principal* to the last, so that it is the *principal* molar which is the largest, and the third, the smallest, and this as well in the upper as in the lower jaw. In the Chimpanzée, this is certainly the arrangement in the upper jaw, whilst in the lower jaw, it approaches slightly that of the Orang-Outang, that is to say, there is a greater equality between the three teeth, the first *posterior* molar being even a little broader than the *principal*. In other respects, if the four divisions of the corona are not just so well defined as in the Simiæ lower in the scale, assuredly there is not the slightest appearance of the small accessary tubercles distinctly observable in

the Gibbon, and on the presence of which Naturalists place great and specific distinctions.

With regard to the dentar alveoli of these Simiæ in the ascending scale, it is easy to perceive that they will necessarily present some corresponding differences; thus, in the upper jaw, the two external holes for the first, and especially for the second of the *anterior* molars, scarcely exist, as well in the Chimpanzée as in the Orang-Outang. The differences are less marked in the alveoli for the other molars, and particularly in the lower jaw.

If, however, proceeding from our type, we descend in the series, we find that the incisive teeth are placed more and more forward, are implanted in a narrow and prolonged parabola, the central pair being more and more predominant over the external pair, which one may easily confirm by comparing the Mangabey with the common Macaque, with the Magot, and especially with the Mandrill, the most imperfect of the Cheiropitheci.

We may make the same observation in directing our observations to the canine teeth, and particularly the upper, which become truly cutting posteriorly, channelled anteriorly, and the enormous development of which reacts on the extremity of the jaws, which become contracted immediately before the increase produced by the implantation of the fangs of these teeth, and even over the first inferior molar, the jaw is, as it were, reversed from before backwards, by the action of the upper canine.

The molar teeth themselves present more and more, in their contracted corona, that disposition to form a transverse, chisel-shaped edge, more or less cutting, and divided into two tubercles; the *principal* and the *posterior* molars are more and more compressed, and as it were fluted on this external aspect; and of the three inferior *posterior* molars, which increase from the *first* to the last (the *principal* being evidently the smallest), the third or last presents a fifth tubercle well developed.

After the differences which we have just pointed out in the teeth, in the descending scale of the Simiæ, we can readily imagine the appearance which the alveoli will present. The chief differences, besides the proportional change, affect the alveolus of the first *anterior* molar in the lower jaw, the two holes of which are much apart; the anterior passes obliquely outside the alveolus of the canines; the alveolus of the last *posterior* molar is enlarged behind its posterior key-hole-shaped cavity, for the reception, or, rather, support of the additional heel.

It now remains for us to point out those differences which the dentar system of the Simiæ present according to age, before arriving at the adult state, and which we have just been engaged in considering.

The Simiæ, like man, have a system of dentition, viz., the milk or temporary, very different from that of the adult or permanent teeth, as well with regard to number as form.

The temporary incisives are those which differ least from the permanent, seeing they are of the same number, and have nearly the same form and proportions; the first are, nevertheless, proportionally still larger *than the permanent teeth*.

If the canines differ neither in number or position, it is not so with regard to their form, which is very different, since they are always very small, scarcely crossing each other, and never curved.

The molars differ as well in number as in form, and not only in their corona but in their fangs.

In the *first epoch*, of which I however possess only one specimen, in the very young Cheiropithecus, there are twelve teeth in all; these include only one molar on each side of each jaw, its corona is very long, but narrow, and as it were composed of two parts, an anterior with three superficial points, and a posterior like a heel with two points; at least this is the case with that of the lower jaw, for unfortunately the analogous tooth of the upper jaw has been lost, there remaining only the alveolar cavities for the reception of two fangs.

In the *second epoch*, the most common condition of the molar teeth in the Simiæ is two temporary molars, in each side of each jaw, when there will be twenty teeth in all as in the human child at 3 or 4 years of age.

The first molar in the upper jaw has two pairs of points, nearly equal, with one advanced anteriorly, a little stronger than the posterior; in the lower jaw the first molar is the same as in the first epoch.

In respect to the posterior or additional molar, it approaches nearer the square form, above and below, and it has two pair of pointed tubercles.

I have observed this state of dentition in the *semnopithecus maurus* and *fuliginosus*.

In the *third epoch*, when the temporary incisives have been replaced by the permanent, both superiorly and inferiorly, there are three molars on each side of each jaw, amongst which are the two above mentioned, in addition to the first *permanent* or principal molar.

I have observed this state in the young Macaque *bonnet-chinois;* in a Guenon *nasique*, &c. &c. In this epoch then there are twenty-four teeth, like the human child at 7 or 8 years old.

The continuation of this condition of dentition appears to be considerable, and very often met with. All the Chimpanzées, and all the Ourang-Outangs, which I have seen alive in Europe, being, as it appeared to me, at this age.

At a litte more advanced age (fourth epoch) in which the temporary incisives have been replaced with permanent *(as in the preceding epoch)* we observe four molars, composed of

the two anterior of the *first* epoch (deciduous teeth) as in that of the *third epoch*, the *principal* (permanent) molar, and the first *posterior* molar. In this the fourth epoch, then, we have twenty-eight teeth in all.

The *anterior* molar, at least in the Simiæ with the narrow sternum, are as yet those of the milk set.

I have observed this arrangement in a *Guenon ascagnes*.

In the *G. nasique*, this arrangement took place only in the lower jaw, in the upper jaw the arrangement corresponded to the third epoch.

In a *fifth epoch*, the two anterior (*temporary*) molars, have been replaced by two permanent molars of a simpler form; in other respects the number is the same, viz., four molars on each side of each jaw. At this period the second posterior molar (or wisdom tooth) is only wanting, the total number of teeth being twenty-eight.

Lastly, in the *sixth epoch*, or that nearest the adult and perfect state, we find all the incisive teeth permanent, as also the five molars on each side of each jaw, making in all thirty-two teeth; but the temporary canines are still present, and their replacement by the permanent canines terminates or completes the dentar changes of the Simiæ.*

With regard to the wearing out or decay of the teeth, this change must commence with the *principal* molars, then the first *posterior* molars, then the two *anterior* molars, and lastly, the last posterior molar, at least in the normal or natural state, for under the habitual use of some peculiar kind of food, we can readily conceive some modifications in the progressive wearing of the teeth will take place.

In the Simiæ with the broad sternum, or those in the ascending scale, the dentar system of the young animal is slightly different.

I have been able to observe its progress in the Gibbons, the Orang-Outangs, and the Chimpanzée.

I am not aware that there exists an epoch in which there is only one molar tooth on each side of each jaw, such as we have seen to exist in the class of animals we have just considered, and, consequently, the first epoch in the higher Apes is unknown to me.

In the *second* epoch, that is, when there are only two molars, the first is, in the Gibbons, &c., an almost simple tooth with a heel (posterior tubercle) well developed; it is then a permanent *anterior* molar, instead of that very complex tooth of the Simiæ with the narrow sternum.

The *second* molar, of early age, is formed of two transverse double tuberculated ridges, very much like the second in the same period of life in the ordinary Simiæ.

In the *third* epoch, these two teeth are increased by the addition of a third posteriorly, evidently much stronger, and which is, in fact, the *principal* permanent molar.

In the *fourth* epoch, the first posterior molar is developed, which makes four molar teeth, on each side of each jaw.

In the *fifth* epoch, the two *anterior* deciduous molars are replaced by two anterior permanent molars, as in the other Simiæ, with the narrow sternum.

Lastly, in the *sixth* epoch, there only remains the canine to be shed and replaced by permanent teeth, to complete the dentar system as it is in the adult.

Thus, we perceive, that the Simiæ, at the top of the scale, present a peculiarity in the form of the deciduous molars, and that peculiarity is not found in the human structure, in which the two deciduous molars have a complex corona, very different from which exists in the Simiæ lower in the scale of organisation.

We have no occasion to point out, that the important differences which the dentar system presents in the course of these combinations, from its first appearance until its complete development, may be perfectly and necessarily traced by the alveolar cavities in the jaws, and that, consequently, we may find these alveoli, indicative of the deciduous state of the teeth alone; and then we can observe, that these alveoli, not only become less numerous, but still less deep.

In another or *second* stage, we may find at once the alveoli of the deciduous teeth all entire, together with a part of the alveoli of the permanent teeth.

And, lastly, a third stage may occur, in which an equal part of the two systems may be present, and which will always be very easily recognised. We may limit ourselves, however, to the giving a description of the state of the alveoli, in the complete stage of youth.

The alveoli of the incisives and canines do not differ much in the young animal from that of the adult, except in having a less diameter, and especially less depth. The alveoli of the molars are also not only less

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* The differences, then, which exist between man and the Simiæ, in respect to their dentition, are extremely slight indeed. Perhaps the most remarkable similarity which exists, is with regard to the two *anterior* molars. We find in the second epoch two molars of large dimensions and great complexity of coronæ, whilst in the fifth epoch we find these have been replaced by two permanent molars of a totally different form and smaller size. The first of these permanent molars is really a very singular tooth, and seems as if modified by the action of the upper canines; but this would be a false deduction, as the permanent canines are still to cut the gum. Thus accurate investigation continually upsets theories, and proves design in the animal economy even where we would scarcely think of looking for it.

deep, but they have besides a slightly different form.

In the Simiæ, with the narrow sternum, the alveolus of the first deciduous molar is very large, and presents, towards its base, three conical holes. But in their second epoch, the arrangement of the alveoli is sufficiently similar to that of the adult, viz., three holes above, and two below, only, arranged closer.

In the Simiæ, with the broad sternum, there is evidently a greater resemblance both with regard to the alveoli, and the coronœ, with what takes place in the species with a narrow sternum, in this respect, that there are three holes above and two below, but still somewhat closer than in the species with the narrow sternum.

To terminate the description of the hard textures, which enter into the organisation of the Simiæ, we have still to speak, according to the plan we have adopted, of three kinds of hard textures, and which we have already described; but we have seen that there are no Dermatostea (bones of the skin) amongst the mammalia, excepting in the Armadillos, nor are there any of the petrostea (internal bones or bulbs), not even in the Cetacea or the Ornithodelphi; but the Enderostea (profound) are present; there is a bone in the penis in all the inferior Simiæ, from the highest organised Guenons, even to the most inferior of the Cheropitheci; this bone is sub-cylindrical, nearly straight, about seven lines in length, by half a line in thickness, in the Magot, *P. Cynomolgos;* whilst that of the *Cebus Apella*, a *Sapajou*, is only about four and a half lines in length, by scarcely half a line in thickness. In the Simiæ, with the broad sternum, or those at the top of the scale, as the Chimpanzée, Orang-Outang, and Gibbon; the bone in the penis does not exist.

The bone of the heart (Enderostea or profound) is not found in any species of this first great class of Mammals. In respect to the Bulbostea, viz, the petrous portion of the temporal bone, we have already alluded to it when speaking of the bones of the head.

LECTURES
ON THE ORGANS OF REPRODUCTION IN THE ANIMAL KINGDOM.

DELIVERED IN THE ROYAL COLLEGE OF SURGEONS, LONDON.

By PROFESSOR OWEN, *F.R.S.*

LECTURE VII.—MAY 5, 1840.

IT has been shown, in the previous lectures, that the doctrines of spontaneous generation, far from being supported by the history of the polygastric infusoria, were positively contradicted by a careful inquiry into their modes of generation. One of the principal and most prominent of the arguments deducible from them is, the repetition of a *fixed and unchangeable form*. All the polygastric infusoria at present known may be referred to one of twenty-two families, of which eleven are provided with an external siliceous case. These genera were recognised and described, with great accuracy, two hundred years back, and they were also familiar to the observations of the great micrographer Leuwenhoek. The siliceous cases of infusoria of ages past are found in great abundance in a fossil state at the present day, and correspond, most accurately, with living genera of the present time. Added to their constancy of form, when we consider their complicated structure, and their perfect powers of generation, we cannot but feel how inadequate any reasoning must be to establish, upon the polygastric infusoria, any sound argument in favour of spontaneous generation.

Failing to prove their opinions by reference to infusoria, theorists next betook themselves to the class entozoa, and here we shall have opportunity of showing that they were equally in error. The lowest of the entozoa, with the exception of some few species, which propagate by gemmation, have distinct male and female organs of generation; in the tape-worm, the organs of both sexes, together with a gland for preparing a distinct protective covering to the ova, are contained in each segment; in the trematoda, or flukes, there exists a complicated hermaphrodite system, and ova, with shells provided with a moveable operculum, which is raised, and falls off as the embryo progresses in its growth. Moreover, the young animal differs from its parent in possessing organs of motion, adapting it to the medium in which it must commence its career, and eyes to guide it in its movements; both of which are lost when the creature fixes itself for its permanent station through life. The nematoid family are diœcious, possessing distinct sexes on different individuals; the testis is an extremely long, simple duct; while the ovaries are also very long, equally simple, and two in number; in the Ascaris lumbricoides, the ovaries, with their ducts, are eight feet in length, and contain ova capable of producing, at one birth, a number of living beings more than three times greater than the whole of the inhabitants of Great Britain. In their extreme fertility the entozoa bear some analogy to the cerealia in the vegetable kingdom, and serve to produce food for more highly-organised beings than themselves.

Another argument against the spontaneous generation of entozoa, is the localisation of the genera. Soëmmering suffered from a tape-worm that was not known in his own country, but was indigenous to the inhabitants of Switzerland, in which country he

spent some years of the early part of his life. Mr. Owen knows of but one instance in which the filaria medinensis has been seen in any others than the natives of countries in which this worm is endemic, and that occurred in an American servant, whose business it was to guard the negroes who were freshly imported from Africa. There cannot be a question, but that in this case a germ from one of the negroes had been conveyed by contact to the skin of the servant. The young of the filaria are peculiarly fitted for piercing the integuments, by being acutely sharpened at one extremity, just as the insect, with its sharp and long ovipositor, is enabled to pierce the thick integuments of animals, and deposit its ova deeply in their tissues. With such considerations as these before us, should we not rather seek to discover the origin of entozoa, by carefully inquiring into their history and peculiar habits, than rest our surmises upon the feeble hypothesis of spontaneous generation.

ANNELIDA.—Among the lowest of the red-blooded worms is the Planaria, a small animal found abundantly in fresh-water ponds. By some it is still classed among the entozoa, although in all its characters it belongs to the annelida, and has been found within the bodies of animals only in very rare instances. Its organisation is so extremely simple, possessing, in every point of its structure, all the elements of the whole, that it is enabled to propagate by means of transverse fission. Trembley observed, that the minutest portions would grow and become a distinct individual when separated from the rest. When it is cut transversely into two halves, the anterior portion will develope a tail, and the posterior a head with two eye-specks; when bisected longitudinally, each lateral half developes its corresponding half; when split, only a part of the way along the middle line, each section developes its corresponding half, and a double-headed monster results; if each of these heads be divided in the same manner, a quadruple-headed monster is produced, and so on. This artificial subdivision serves as a good illustration of the permanent partial fission of the polypi. The planaria possesses distinct male and female organs on the same individual, as complex in structure as those of the trematoda; in the male there is a double testis, with vava deferentia terminating in an intromittent organ; and in the female, ovaries; a perfect coitus takes place between the individuals, and a glutinous substance is secreted around the ova, by which they are attached to the stems and leaves of water plants. Another minute genus of the annelida, the Nais, possessed of distinct generative organs, is also remarkable for propagation by transverse fission. The changes by which this division are effected have been carefully observed by F. Müller. In the first instance, the alimentary canal commences to enlarge at a particular point, and currents of blood are directed obliquely between the enlarged part and the integument; a constriction then occurs between two of the segments, a proboscis and two eyes are formed, and the constriction increases to complete fission; this process may be taking place at the same time at three or four different points, so that we are presented with the anomaly of four or five generations in a single animal, nourished by one mouth, and furnished with a single anus, the former being upon the anterior extremity of the parent, and the latter on the offspring of the fourth generation; the generative organs resemble those of the leech.

In the male of the Leech, nine small oval bodies are seen on each side of the middle line upon the ventral surface of the animal, and beneath the alimentary canal. These are the testes; there proceeds from each gland a short duct, the vas deferens, which opens on each side into a long common canal, passing longitudinally forwards to an organ which would seem to represent the prostate gland; from the prostate the two ducts proceed to the angles of a pyriform receptacle, the vesicula seminalis. In the female there are two ovaries, which communicate by means of two oviducts, with a pyriform uterine sac; the penis is placed on the twenty-fourth segment of the body, the vulva on the twenty-ninth, and the coitus is reciprocal. The changes which occur upon the exclusion of the ova in the rivulet leech have been attentively examined by Dr. Johnson, and are described in the "Philosophical Transactions." On the day after the coitus, a double constriction is observed around the body of the leech, and the intermediate segments gradually change their colour, and become whitish and opake; this opake ring is shortly observed to consist of ova, inclosed in a white glutinous substance; after remaining a short time, the leech exerts itself to get rid of this ring, bending its body rapidly and violently, and applying its head to the part as if to loosen it from its adhesion to the skin; it succeeds, eventually, in its object, and withdraws its body from the ring, leaving it behind in the form of a cocoon, open at both extremities. In the medicinal leech these cocoons are often found in the water in which they have been kept, and are seen to be composed of twelve or fourteen ova, inclosed in a nidamental substance. Wagner found the ova to contain all the essential parts of the ovum of higher animals, and to lose gradually the germinal sac; and Mr. Owen had observed the vitelline granules to assume a radiated arrangement during the progress of development. The young, after their escape from the ova, quit the cocoon through the openings left by the body of the animal.

In the common Earth-worm the testes are

two on each side, and the ovaries four, in the same individual. The common vas deferens is short, and terminates by a very small pore on the fifteenth or sixteenth ring from the head; the ovaries are simple pyriform glandular sacs. Upon the sixteenth segment are two small transverse slits, or vulvæ, situated one on each side, and communicating with the ovaries; sometimes there are two vulvæ on one side, and one only on the other. Besides the proper organs of generation, there is developed around the worm, towards the latter end of summer, a thick and broad belt, which is called the clitella; the clitella serves as a sucking apparatus, by means of which the worms are connected with each other during the coitus. Upon the surface of the clitella are produced two small filamentary processes, which seem to serve the office of organs of excitement; they are imperforate, have no connection with internal glandular organs, and disappear immediately after the coitus. The seminal fluid contains an abundance of spermatozoa. The earth-worm, in its mode of reproduction, bears a very close analogy to plants; the vulvæ are not intended for the transmission of the ova, but merely serve as tubes of conduction of the seminal fluid to the ova. In this peculiar disposition they may be compared to the minute canals in the stigma of a flower, through which the fertilising atoms are conveyed to the seedvessel. In the month of July the ovarium, filled by the contained ova, like the seedvessel with its ripened fruit, bursts, by a process similar to the dehiscence of plants. The ova are dispersed within the interior of the animal, and pass backwards between the integument and the intestine to the anal receptacle at its posterior extremity; in their progress they gradually undergo their development, and are expelled from the parent, either in the form of completely formed worms, or surrounded by a dense and tough case, which gives them the character of pupæ. Whether they are produced in the living or in the pupal form, depends upon the nature of the soil in which the worms are found; in a light and loose soil, they quit the parent, prepared to act for themselves; but in a tough, clayey soil, they continue the pupal form for some time, and undergo a still further development to fit them for an independent existence.

The marine Annelida do not possess sufficient interest to deserve an especial consideration.

Cirripedia.—Until very lately the most erroneous notions have been entertained with regard to the Cirripedia, or Barnacles, from the circumstance of their natural affinities not having been perfectly considered. They present the peculiarity of being inclosed in a multivalve shell, but are provided with distinct articulated members, and are divisible into two groups, the *sessile* and *pedunculate*. In their organs of generation they are hermaphrodite, having, like the leech and earth worm, distinct sexes upon the same individual. The testes, situated near the hepatic cœca, are lobulated and granular. Their secretion is conveyed by numerous ducts uniting to form a single canal, which terminates at the base of an extensile organ. This organ was thought, by Hunter, to be the penis; the recent researches of Wagner have confirmed the fact, and have established, beyond dispute, the true nature of the male glandular organs, which were described, by previous anatomists, as ovaria. The ovary, in the pedunculate barnacles, is situated in the stem; in the sessile group it is attached to the base of the shell; in the latter, the common deferential, and ovarian duct is longer than in the former. Impregnation is performed by the same individual; but when the animals are collected in clusters, their impregnation is probably reciprocal.

Had the Barnacle produced young similar in instincts to the parent, they would have destroyed their means of support by covering and killing the animals upon which they fixed themselves for their growth, or would have buried the parents by the new and numerous accessions to their numbers. As a provision against the possibility of such accidents, we find them, in their embryonic state, possessed of very different habits to the parent, and undergoing a remarkable metamorphosis. Dr. Vaughan Thomson obtained, in the Bay of Cork, numerous minute crustacea, having two lateral folds of integument, four locomotive ciliated organs, and distinct eyes. On observing these little creatures during the progress of their growth, he found that they cast their outer covering and locomotive organs, and became converted into sessile barnacles. Burmeister, in the young of the pedunculate group, observed four legs on each side. During the process of ecdysis, or moulting, the anterior pair became converted into the organ of attachment; the single eye disappeared, and the lateral folds, or mantle, developed the shell. When the researches of Dr. Thomson were first made known they were unique; but at the present day we are acquainted with many illustrations of the same phenomenon; as in the Lernææ, and in the Trematoid Entozoa.

ON THE

CONSTITUTIONAL ORIGIN OF SOME DISEASES OF THE SKIN.

By ERASMUS WILSON, *Esq., Surgeon to the St. Pancras Infirmary.*

THE diseases of the skin form a field of inquiry which all writers have acknowledged to be one of great difficulty and obscurity, and which often seems to set at naught the best-directed efforts of the surgeon. It was a feeling of this nature, combined with the interest which I ever felt in pursuing the structure and organisation, and watching the physiological action of the dermoid tissues, that first induced me to engage in a subject so fraught with obstacles and disappointments, and offering so few and uncertain guides for my direction. I should but be repeating the daily experience of every practitioner in medicine, were I to affirm that I have been very frequently foiled in my best attempts to confer benefit in these cases; but I must, at the same time, acknowledge that I have learnt many and most important facts, which I expect will take the place of principles in my future practice.

My great and respected teacher, Mr. Abernethy, was the first of modern surgeons to call the attention of the profession, strongly and forcibly, to the "constitutional origin and treatment of local diseases;" and imbibing those early principles, although not led away by an imposing theory, I have been urged into a conviction of the truth of his views in reference to diseases of the skin. As a present illustration of the truth of these remarks, I will pass over many cases, pointing to the same fact, which I have recorded in my case-book, and select one which has very recently been under treatment, and is now quite recovered.

In walking through the school-room of a private charity I remarked a little girl, nine years of age, who was affected with porrigo favosa in its first stage; the rest of the children, about thirty in number, were quite free from any cutaneous affection. On making inquiry, I found that this child was the daughter of an exceedingly cleanly and attentive mother, and it was looked upon as remarkable by the school mistress that she, of all the children in the school, should be the subject of a disease which is too generally regarded as the offspring of dirt and neglect. The mother told me that the child had been liable, from her infancy, to frequent attacks of pain and swimmings in the head; she had two younger children, a girl, three years of age, and a fine healthy boy. She had observed, since the eldest child was sent home, that the other little girl had likewise some affection of the head which she had not previously seen, although it was her habit to wash their heads well every night before sending them to bed. The digestive organs of both children were undisturbed. I had the hair cut short in both, and also introduced a seton through the nape of the neck of the elder, pursuing the general mode of treatment which I have before pointed out in Vol. I., 1839-40, page 795, of THE LANCET. In twenty-eight days the pustular disease had disappeared in both children; the surface of the scalp was no longer puffy and inflamed; the hairs had ceased to break off and fall from their follicles, and new hair was observed to be growing in several situations. The health of the elder child was better than it had before been for several years, and the swimmings and pains in the head were entirely gone. I ordered the seton to be continued for another fortnight.

About a week after taking leave of the eldest girl, and assenting to her return to school, she was again brought to me, suffering from an eruption of lichen over many parts of the body, particularly around the wrists, the bend of the elbows, on the chest, abdomen, and back, and in the popliteal regions. The affection was extremely irritating, destroyed her rest at night, and her general health became disturbed. I gave her aperient medicines, prescribed frequent ablutions in the tepid bath, and ordered a liniment, containing creosote, to be rubbed over the entire surface of the body. The younger child, and the maid servant, had a similar attack; but under this treatment they all recovered in the course of ten days.

After another week the mother came again to me in great distress, saying, that the disease had broken out a-fresh, and that she had been persuaded to go to a physician, who told her that her children were suffering from *inveterate itch*, and that unless she immediately procured the means of obtaining some sulphur baths for them, that the whole street, and, indeed, the whole of London, would be infallibly exterminated by the disease. The woman described the doctor's alarm at the presence of the children in the most amusing manner, and said, that he seemed to be in an agony at the necessity of breathing the same atmosphere with them. He started from his seat in horror, when the youngest child attempted to pick up a morsel of paper from off the floor, and desired that they would quit the house as soon as possible, and not touch any article of furniture, or the door-handles, in their passage out. I trust that he fumigated his apartments well, and put his chairs in quarantine after this dreadful cutaneous visitation.

I scolded the mother soundly for going to a doctor without first consulting me; but thought, at the same time, that she was sufficiently punished in the reception which her ignorance had received. The disease

had now, in the elder child, merged in a severe attack of eczema impetiginodes; the younger girl was perfectly well, and had scarcely any trace of the lichen remaining. I assured the mother of the needlessness of her alarm; that the disease was perfectly harmless, as far as contagion was concerned; and that it was, probably, advantageous to the child, as a last outbreak of some general constitutional disturbance, which developed its violence upon the skin, in place of some internal organ, and one more immediately important to life.

The eczema first appeared upon the forearms and hands, particularly about the wrists, and then proceeded to the legs and ankles, presenting its ordinary characters; there were one or two pustules formed even upon the sole of one foot. I ordered the treatment which I have before used so successfully in this disease, and recorded in THE LANCET, Vol. I, 1839—40, page 882, and she is now completely recovered.

In the above case will be found several points worthy of remark. The first of these relates to the contagious nature of porrigo; and we have before us an instance, in which one single child, in a whole school, is affected, without a trace of the disease existing, either before or since, upon any of the the others. Secondly, a sister of the same child, with whom she sleeps, is affected, whether by direct contagious communication or by epidemic influence, is yet unknown. Thirdly, that another child, a brother of these children, and also sleeping and constantly playing with them, is wholly unaffected. And, fourthly, that a maid-servant, nursing the children, suffers from lichen, a non-contagious disorder, at the same time with two of them. The second point for consideration is, the occurrence of three diseases of the skin, supposed to be wholly distinct from each other, viz., of a pustulous, papulous, and vesiculo-pustulous disease upon the same child, in rapid and almost continuous succession. Surely this must betoken a constitutional cause, and would indicate that a mere local treatment in diseases of the skin is not founded upon correct physiological principles.

Upper Charlotte-street, Fitzroy-square.

HISTORY OF A CASE OF

DEAFNESS EFFECTUALLY CURED

BY PUNCTURE OF THE MEMBRANA TYMPANI.

To the Editor of THE LANCET.

SIR:—As the deafness here described depended in part upon an *original defect* in one ear, and in part on the effects of disease in the other, attention must be paid to the distinction between the two. The tympanum of my left ear was, I believe, fissured from birth; for when a child I could force air from the *outside* through the Eustachian tube. I did not then know the nature of this peculiarity; neither was I sensible of any degree of deafness, although it may probably have existed on that side, but the other ear being perfect I felt no inconvenience. When thirteen years old I had scarlet fever very severely, with sore throat, and inflammation of the glands of the neck; I believe, also, that suppuration took place in both ears, though not to any great degree. On recovering from the fever I became very deaf with both ears. I now found that I could not force air as before, through the left the passage was impeded. Instinct, however, prompted me to attempt it whenever I listened, as I derived a little benefit from inflating the tympanic cavity. I now learned to do this from the *inside*, by compressing the nostrils, &c. At this period, and for many years after, I was annoyed with noises in the ears, such as the ringing of bells, explosive sounds, and, in particular, with arterial beating in the *right* ear. I was taken to the late Dr. Wishart, of Edinburgh, who syringed the ears with soap and water, gave mild aperients, and ordered the use of almond oil. These applications produced no effects. After the lapse of some months I was sent to the late Dr. John Gordon, who did not discover the fissure in the membrana tympani, and doubted my statement that I could blow air through it. He syringed the ears with lime-water, which produced a confused sensation on my head, but did not improve my powers of hearing.

I was next taken to Mr. Guthrie, an army-surgeon, in London. He recommended blisters behind the ears; I submitted to one only, so that his plan had not a fair trial. From the age of sixteen to twenty-one I went on pretty much in this way. The opening in the left ear, and the Eustachian tube, gradually became more free from obstruction, so that I could always pass air through them, and the hearing of that ear gradually improved. It returned, I believe, to the state in which it was before the scarlet-fever, an imperfect organ, but still capable of service. On the other hand, the hearing of the right ear became more dull, so that at last I made no use of it, but attended only to the more perfect perceptions of the other. I may here mention, that both have always been good musical ears.

When about the age of twenty-one, having obtained a knowledge of the anatomy of the ear, and heard of the operation of puncturing the tympanum, I was anxious to have it tried upon my right, or useless ear. I had a feeling that if the tympanum were pierced, that ear would be brought up to the standard of the other. Having

understood that Sir Astley Cooper first brought this operation into notice, I went to him. He probed the ear, but did not recommend the operation; he thought the ear might improve with time, and recommended patience. I was disappointed at this result; I did not then like to press my own views of the case. I waited three years more, and then, resolving to have something done, I went back to Dr. Wishart, and requested him to puncture the membrani tympani of my right ear. He consented, and pierced the membrane with a flat needle, while an assistant held my head. The feeling was little more than the prick of a pin; I could easily force air through the aperture, and could hear the notes of a pianoforte, a sound that I had been unable to appreciate with that side of the head for many years. I found, however, that the aperture was much smaller than I could have wished, and the improvement of hearing seemed proportioned to its size. A little inflammation was excited, the aperture diminished, and in less than a fortnight completely closed. I then applied to Dr. Wishart to repeat the operation. He waited two or three months, when he again performed it with the same success, and also with the same result, for it closed up as before. I next requested him to pass a trocar through the membrane; this he declined to do, and told me that his advice was to let it alone.

I then went to Mr. Liston (now of London), and, telling him what had been done, I asked him to pas a trocar. He consented, and performed the operation. The pain was nothing; the turning round of the instrument in the wound was, however, very unpleasant; a drop or two of blood followed. I was now repaid, for I found that I had got as effectual a passage through the one ear as the other, and the hearing of the right ear seemed now as good as that of the left. A little more inflammation came on than in the two former cases; but not to a troublesome degree. I avoided cold; but in a fortnight the opening closed, and the ear returned to its former state. Mr. Liston waited about a month before he would repeat what he had done. The result was the same; the wound made by the trocar closed as before. I now asked if he would object to use caustic after piercing? he said, "No;" so, after a month's lapse, he used the trocar, and then introduced a stick armed with caustic. The pressure and friction of this substance against the wound was very painful. More inflammation followed this than the former operations, but it was never very great; the result was as formerly, the wound healed up. After some time the trocar and caustic were tried again, and with the same result. I have now related four operations by Mr. Liston, and I have another to mention, which makes five; but I am not sure that they may not have amounted to six, but having lost the notes which I took at the time I cannot be sure.

This liability to heal up confirmed my belief that the membrane was too thick and vascular. I now asked Mr. Liston* if he would use a punch-shaped instrument, with a circular cutting edge; he had no objections if it could be made. He was not aware, as far as I remember, that such an instrument had been used before, though it has been proposed by writers on the subject. I described it to an instrument-maker, who said it could be made, and Mr. Liston gave him the dimensions. This instrument was then used, and the operation gave little or no pain; certainly less than I have felt from pressure with a probe. Mr. Liston thought that only a segment of a circle was cut. A flap was certainly cut, like the valve in a pair of bellows; this, as it shrivelled, I could blow through the aperture and back again for some days, when it disappeared. The aperture remained open, and has done so for more than ten years; and the hearing became, and has continued, as good as that of the ear which was naturally defective.

My experience leads me to think, that when an opening is to be made in the membrane of the tympanum, it should be of considerable size, for I felt that the needle aperture was too small to serve my purpose. It may also be inferred from this statement, that the last-named instrument is a valuable one, and perhaps to be preferred.

I shall conclude with the mention of a fact, which seems curious. Some days after the membrane had healed, after one of Mr. Liston's operations, I chanced to be shooting. In firing a shot I blew out the *bush* of the gun. This accident, I have heard, sometimes causes the loss of an eye; in my case the explosion from the touch-hole ruptured, with violence, the newly-healed wound in the ear. In a few days it healed, as on former occasions, without any bad effects. I would also state that I have never had earach; and, notwithstanding their condition, my ears are not particularly liable to cold. They do not in general need the defence, very commonly adopted, of being stuffed with cotton. I find that precaution only necessary under

* We have seen a beautiful and ingenious little instrument which Mr. Liston uses at present for this purpose. It consists of a screw with a very delicate spire, by means of which the membrane is fixed; while the turning of a small cylinder, with a sharp cutting edge, cuts out a circular portion. After the section, the separated piece being retained by the screw, is easily removed.—Sub-Ed. L.

continued exposure to a cold wind. The sounds which used to annoy me have almost ceased; and the arterial beating in the right ear has been long since gone.

A. W.

Edinburgh, May 1, 1840.

SLAUGHTERING ANIMALS FOR FOOD WITHOUT PAIN.

To the Editor of THE LANCET.

SIR:—I have addressed the accompanying letter to the Society for the Prevention of Cruelty to Animals: will you oblige me by giving it a place in your Journal. I am, Sir, your obedient servant,

A RETIRED PHYSICIAN.

GENTLEMEN:—The praiseworthy objects for which your Society has been instituted can never be fully attained, unless the destruction of animals for the sustenance of man be effected in a very different manner from that which is customary at present. The amount of pain in a London slaughter-house, during one week, far outweighs the sufferings from incidental cruelties practised on animals throughout the United Kingdom during a year. The occupation of a butcher necessarily renders him callous to the feelings of the animal creation, and prepares him and his assistants for the exercise of those occasional cruelties which call for your interposition to prevent or punish; and there is too much knowledge of the fact, that persons who are in the daily habit of witnessing the painful deaths of animals, are deficient in sympathy for their fellow-creatures.

Some benevolent persons have condemned the general custom of mankind, living so much on the flesh of animals, like the beasts of prey, and have consequently restricted themselves to a vegetable diet, and their example would be followed by many others if they were to witness the scenes of a slaughter-house; but if means were to be devised for extinguishing life without pain, the multiplying of animals for the purpose of food would produce a greater amount of enjoyment, than if the creatures were to be fewer in number, and allowed to perish by accident, disease, or natural infirmity.

Physiology and chemistry teach us the means of removing many moral, as well as physical evils, and of these one of the most valuable, is the mode of abolishing the agony of death, in the millions of animals employed for the sustenance of man. It has been long known that the inhalation of carbonic acid gas, unmixed with any other air, is instantaneously fatal, without occasioning any struggles, or sensation of pain. When the experiment is properly conducted, the interval of time between the life and the death of the individual is too short to be appreciable. The life of the internal organs, however, does not immediately become extinct. The heart continues to beat long enough to admit of the abstraction of the blood, which is found to be capable of coagulating, and the muscles also become rigid—a proof that the respiration of carbonic acid gas destroys life through the medium of the nervous system, and that it has no tendency to decompose either the fluids or the solids of the animal body, so as to thereby render them unfit for food.

If the carbonic acid gas were to be used on a large scale in slaughter-houses, it could be obtained in any quantity, and without expence, from the different manufacturing processes which generate it; and might be kept for use in varnished bladders, or bags of Indian-rubber cloth, from which it might be transmitted into bags of the same material, tied over the mouth and nostrils of the animal, the discharge of the gas being regulated by stop-cocks. But if the mode of slaughtering I have recommended should come into general use, many contrivances would be made which would render the application of the gas easy and safe to the operator, and expeditious with regard to animals intended to be killed.

It is sufficient, Gentlemen, to draw your attention to this subject. Your interest will naturally be excited by a matter so intimately connected with the objects of your Institution.

[*** We are not authorised, at present, to communicate the name of the writer of the foregoing letter; but we may state that he has occupied a high station in the profession for many years, and that his name, if announced, would ensure for his communication, the utmost respect.]—SUB. ED. L.

SEVERE CASE OF HÆMATEMESIS.

SULPHATE OF COPPER IN HOOPING-COUGH.

To the Editor of THE LANCET.

SIR:—I beg to send the following for insertion in your valuable columns. I am, Sir, your obedient servant,

PYE HENRY CHAVASSE.

Birmingham, May 9, 1840.

HÆMATEMESIS.

April 19, 1840. I was sent for at eight o'clock this morning, in great haste, to attend John Jones, who was vomiting blood. The vomiting had commenced yesterday morning. He has had three or four attacks since then, and has brought up in that space of time (twenty-four hours) at least half a gallon of blood. I found him excessively weak and exhausted; his countenance death-like; his

skin covered with a cold, clammy perspiration; his pulse quick, and extremely feeble; he has pain and tenderness over the scorbiculus cordis; he has a trifling cough, which appears to proceed from a cold he has been labouring under, but in every other respect the lungs and their membranes appear perfectly healthy. My patient has been a very hard drinker, so much so, that he has almost subsisted on drink alone; the consequence has been that he has been labouring under symptoms of indigestion. The following is the treatment:—

Superacetate of lead, grs. xxiv;
Powdered opium, grs. viij;
Mucilage of gum arabic, a sufficient quantity to make eight pills; one to be taken every four hours.

The room to be kept cool; the patient to be kept very quiet, and to have nothing but cold water and cold gruel to drink, and to have no solid food whatever.

9 o'clock, P.M. I found him much better; he has had only one return of vomiting of blood, and that only small in quantity, since I saw him this morning; the pain and tenderness of stomach much relieved; pulse reduced to 80; tongue clean; expresses himself as being much better. Continue the treatment.

20. Very much better. Continue the treatment.

21. Rapidly improving; no return of the vomiting; tongue clean; tenderness of the stomach nearly gone; pulse 72; bowels confined. Discontinue the pills, but proceed with the low diet, and take the following mixture:—

Sulphate of magnesia, ℥j;
Carbonate of potash, ℥ss;
Simple syrup, ℥j;
Water, ℥vi, M. Two tablespoonfuls of the mixture to be taken with one of lemon-juice every three hours, whilst effervescing.

22. Still better. Continue the mixture, but allow him weak broth.

24. He has slight pain over the region of the liver; in every other respect (excepting weakness) he feels well. Discontinue the mixture, and take the following pills and mixture:—

Compound extract of colocynth,
Blue pill, of each, ℥ss. To make twelve pills; two to be taken every night at bed-time.

Compound decoction of aloes, ℥iij;
Peppermint water, ℥iij;
Camphor mixture, ℥j; M. A wineglassful to be taken three times a day.

May 1. He has been rapidly improving since last report, and now feels better than he has done for the last twelve months.

SMALL DOSES OF SULPHATE OF COPPER IN HOOPING-COUGH.

I wish to call the attention of my medical brethren, through your medium, to the great value of *small* doses of sulphate of copper in hooping-cough. I have used it in numerous cases, and with the happiest result. The form I generally order is the following:—

Sulphate of copper, ½gr.;
Syrup of poppies, ℥ss;
Aniseed water, ℥ss; M. One or two teaspoonfuls (according to the age of the child) to be taken every four hours.

INQUEST AT ST. BARTHOLOMEW'S HOSPITAL.

To the Editor of THE LANCET.

SIR:—I forward to you an account of an inquest, which indicates some culpability which the coroner appeared to be unable or unwilling to detect,—most probably the former; a notice of it should be added to the records of "incomplete inquests."

On Tuesday, the 28th of April, an inquest was held at St. Bartholomew's Hospital, on the body of John Ryan, a child, three years old, of Aldersgate-street. The boy was suffering under an attack of measles, and was attended by Dr. Farre, of the Aldersgate Dispensary, who prescribed mercury, in small doses. Immediately after the administration of the medicine, the child got worse; the mouth became inflamed, dark, and discoloured, the teeth dropped out, and he died in great agony. A general practitioner was called in to attend, and he pronounced that the child had been excessively salivated. In my examination at the inquest, however, he seemed loth to inculpate the medical officers of the Institution.

The party at the Dispensary, who made up the recipe, never weighed the calomel according to the evidence of the mother.

I felt great regret on this occasion that we had not a competent coroner, possessing a knowledge of medicine, to direct the investigation into its proper channel. Dr. Farre was much annoyed at the neglect of the compounder, seemingly contrary to the wishes of the former.

We recorded the following verdict:—"That the prescription was neglectfully compounded, and that the child died from an overdose of mercury." I am, Sir,

ONE OF THE JURY.

*** In answer to a private question on this subject, it may be stated, that in all Middlesex there has not, in the last election for coroner, been found so determined a body of opponents to the appointment of a coroner possessing medical knowledge, as in St. Bartholomew's Hospital. The foregoing report has been authenticated.

THE LANCET.

London, Saturday, May 30, 1840.

THE discovery of JENNER has been in the possession of the world for more than forty years. All the nations of Europe have availed themselves, more or less, of its advantages. It is an effectual check to the ravages of small-pox. If the entire population were protected by vaccination, it is not disputed now that a certain number would be cut off by small-pox; but experience has proved, that, of the persons vaccinated, few are attacked by that fatal malady, and that in the exceptional cases the affection is modified, rarely leaving permanent cicatrices, disfigurement, or blindness, and seldom terminating fatally. Is it creditable, then, to England, that, since the causes of death have been registered, *twelve thousand* deaths a-year should have been ascribed to small-pox? Is it to be endured, that nearly *twenty thousand* infants and adults in Great Britain and Ireland should fall victims, year after year, to this fell destroyer, which JENNER has taught us to disarm? It is admitted that an effort should be made by the Legislature to extend the practice of vaccination; and while the public are prepared for this step by the recent epidemic, the call for it by the medical profession is general.

The question then, is, In what way is the practice of vaccination to be extended? Two plans are before the public, and two Bills lie on the table of the House of Commons, both of which profess to have similar objects in view. The readers of THE LANCET have had an opportunity of comparing the two Bills. Their modes of operation are founded upon totally different principles, and it may be useful to place them, at the present moment, in juxta-position, that the contrast may be distinctly understood.

Vaccination may be rendered *compulsory*; parents may be bound, by statute, to secure the protection of their offspring, for it is a kind of infanticide to suffer children to die whom a slight operation would save. Some Governments of the Continent have acted on this maxim, have appointed public vaccinators, and have enforced vaccination.

The principle of compulsory vaccination is intelligible, and may be supported by many arguments; but the objections to it preponderate. At any rate, they should make us hesitate in recommending the extreme course, before giving other means, more in conformity with the feelings and institutions of the country, a fair trial.

A considerable proportion of the children born in the United Kingdom are never vaccinated; a fifth, perhaps, of the people remain unprotected. The consequence is, that nearly twenty thousand lives are sacrificed annually. Now, the law punishes infanticide. The life of the unborn child is not at the disposal of the mother. The English father cannot, as the Roman father could by the Roman law, dispose of his child's life. May not the parent, then, be compelled, on this principle, to have his child vaccinated? Should he not be punished, fined, or imprisoned, for neglecting a clear, obvious duty, involving the destruction of human life? Would there be greater hardship in fining the parent, through whose negligence a child died, than in imprisoning or transporting a man who stole a sheep or shot a partridge? Would there be any impropriety in fining a householder who had an unvaccinated infant under his roof? Would it be a greater infringement of liberty, or more repugnant to him, to pay a fine in this shape than to pay a tax of 14s. for a dog, or, £1 8s. 9d. for keeping a horse, or a fine for giving a receipt on unstamped paper?

Vaccination may be rendered compulsory without violating any constitutional principle; it would be absurd to maintain the contrary. But a measure founded on the compulsory principle is inexpedient, and it appears to us to be unnecessary.

The children of the wealthy and middle classes are invariably vaccinated. Few remain unprotected at the end of six months,

so that small-pox is generated, and finds the majority of its victims, among the poor, and pauper classes. Now, let us, for a moment, visit a poor family in which the children are unvaccinated, and a death from small-pox has occurred. In the presence of the mother, we ask, what steps should have been taken, or is it desirable to take, to remedy the neglect? Fine or imprison her, say the advocates of compulsion. But why has she neglected her duty? Question her, and any or all of the following reasons may be alleged:—She believes that cutting for the cow-pox is a bad thing altogether; that it introduces "humours" into the child's blood; that it imparts a "beast's" disease, and that it is much better to let things take their natural course. Or she wished to have it done, and liked to have Doctor ANDREWS, who lived in the same street, but could not afford to pay him, and did not like to trouble him, though he offered to do it for nothing. Then the place where they inoculated children gratis was at a great distance, and they took the matter from all sorts of scabby little things; besides, she did not like the doctor there, who was a great fool, or very rough, and unfeeling.

Now, to fine or imprison a poor, ignorant individual, entertaining such perverse opinions, would neither enlighten her mind, nor dissipate her prejudices; but make her cling to error with a morbid tenacity. Besides, who would give the information,—who would undertake the task of public prosecutor, and demand the punishment?

Mr. DODD tells us, in his letter, which is otherwise interesting, that he would offer the recusant mother *sixpence*; and we have been informed, that the Corporation of Norwich formerly gave *half-a-crown* to the mothers for every child vaccinated. It may not have been amiss for the Corporation of Norwich to dispose of a few half-crowns in gifts to poor mothers; but the grave offer of a bribe of sixpence, or six shillings, to the mother, under the circumstances, would be recognising a revolting principle. Mr. DODD will perceive this, upon a moment's reflection. The mother believes that vaccination will do the child harm; can her objections be overcome by a bribe of *sixpence*? Or, if she take the bribe, will she not be violating the injunctions of her conscience, and, according to her own view, be selling her infant's life, and sacrificing its health, for money? The mother believes, on the other hand, that vaccination is salutary, and may save life; is it to be assumed that she will do that for *sixpence* which she will not do to protect her offspring from small-pox, or from the chance of its fatal consequence? The representation of the mother and child has in it something divine; and the mixed tenderness, affection, and disinterestedness in their relationship, have been the topics of some of the purest and loftiest works of art. "Had I a city," said a great physiologist,* "within its precincts "should be raised that emblem of domestic "happiness—a mother suckling her child." No one can witness a number of anxious mothers, with children in their arms, around a vaccinator, without gratification; and, in the hands of a good artist, it has often occurred to us that a group, with JENNER in the centre, might form the subject of a great national picture, to commemorate the beneficent achievements of his mind. But if the group were collected to avoid a punishment, or to gain sixpences, and the radiant play of maternal affection were converted into a sullen ill-will, or a sinister expression of meanness and mendicancy, who could look on the scene with satisfaction, or without disgust?

The authors of the two Bills agree in this, that the parents should neither be compelled to have their children vaccinated, nor paid for performing their duty; but for different reasons. The author of the one Bill trusts that, eventually, every object will be accomplished without compulsion or bribery; the authors of the other Bill seem to have cared less for the extension of vaccination than for the extension of the grasping, domineer-

* Gall.

ing powers of the Poor-Law Commissioners. The Small-Pox Prevention Bill is founded upon the principle, that inoculation is too culpable to be longer tolerated; that no obstacle to vaccination should exist, and that every facility should be created for encouraging the adoption of JENNER's operation. Its principle is persuasion.

The parents will be relieved from the expense of the operation. The poor man will not have to take bread from the mouths of his children to pay for vaccination. The mother may carry her infant to the nearest legally-qualified practitioner, or to the medical man in whom the parents have the greatest confidence. She may apply for an "Order" at any time when it may be most convenient. Prejudices will be gradually overcome, by the statements and well-timed expostulations of members of the medical profession, or other intelligent persons. A little discussion will upset the popular theory of "humours," which, it must be borne in mind, *originated in the medical schools*. It will be sufficient to *explain* to the female physiologists of St. Giles's, or of the Welch mountains, the principles of the new doctrine, to secure its transmission to the third and fourth generations; or to remind them that their wealthy and intelligent neighbours have their children vaccinated. The vaccination of the young Prince, whom the country is led to expect, would be an impressive example; and, with an illustration or two from nature of the fatal effects of neglect, might be decisive; such, for instance, as that Mrs. M. had a nice little boy, quite well on Monday, who caught small-pox, died, and was in his coffin on Saturday; that her little girl was blinded; and that neither had been cut for the cow-pox; while, in the same court, only one of Mrs. L.'s children, who had been vaccinated, had small-pox, and he had it very slightly, and was not ill a week, &c. &c.

If the parents yielded to such reasons, two good objects would be accomplished; the children would be ever afterwards taken to the vaccinator, while the parents' minds, instead of being debased, would be enlightened and morally improved; for, in complying, they would be cultivating their sense of duty, and the sentiment of parental affection.

The cordial co-operation of the medical profession would be secured by the Small-Pox Prevention Bill. For as it would confer no monopoly, it would leave the relations of private practice undisturbed. So far as vaccination is concerned, the poor would be in the condition of private patients, with a moderate rate of remuneration secured to the practitioner.

It would be acceptable to all who were entrusted with carrying the provisions of the Bill into execution. The very reverse may be predicated of the Bill from the Lords: it would be offensive to the medical practitioners, as it would place them at the mercy of the Poor-Law Commissioners, who would have the power to annul *contracts*, the odious principle of which forms the basis of the measure, and to issue *regulations* respecting vaccination. It would be grossly insulting to the whole body of the medical profession, and of little advantage to science, to insist that the Vaccination *Reports* should be referred to the Poor-Law Commissioners, who know less of that subject even than of the principles or practice of political justice.

The children of many "independent" persons, not absolutely paupers, would be vaccinated under the law; and if the Union officer had the monopoly of the public vaccination in his hands, he would inevitably be employed, in the end, to vaccinate the children of the middle and wealthy classes. This would be an introduction *to their families, that is, to the patients of the other medical practitioners of the district*, and would be an unfair, vexatious interference with private practice. In self-defence, the Union vaccinators would be zealously opposed.

Then, to place vaccination in the hands of the Somerset-house Commissioners would not be the way to render JENNER's discovery POPULAR. It would not be concili-

ating the minds of the poor; not consulting their wishes; but be adding to the existing prejudices against vaccination, the hated association with their fancied oppressors. What effect would it have on the prejudiced mother, to tell her that vaccination was recommended by the Poor-Law Commissioners? Why, it would suggest some malicious design on the child's life: the very mention of their names would be enough—the names which she familiarly associates with hard-heartedness, and might use as a bugbear to her boys—the ordinary climax of maternal terrors being, in much likelihood, "the Poor-Law Commissioners," in many neighbourhoods, where, for "I will call the beadle to you"—or "Old Nick will take you,"—"the Poor-Law Commissioners shall have you,"—may ring in the horror struck urchin's ears. And the agents of the Poor-Law Commissioners are to cut the children of these parents for cow-pox, which, in other hands, they scarcely yet believe to be the result of an innocent operation!

Admit that the anti-poor-law prejudices are exaggerated—and we do not suppose that they are true to the letter—still they widely exist, and actuate the very classes who require vaccination. No practical statesman, therefore, who has the health of the poor at heart, will support a Bill that runs directly counter to popular feeling, and entrusts vaccination to the Poor-Law Commissioners; or will refuse to adopt a measure which is both supported by the medical profession, and calculated to confer a benefit upon the people in the manner that is agreeable o eir feelings.

Of the two Bills now before the House of Commons, that from the Lords, having been read a second time, the Committee on the Bill has been deferred until Wednesday, the 17th of June. The SMALL-POX PREVENTION BILL was read a second time on the 22nd instant, committed, also, for the 17th of June, giving abundant time for making representations, by Petition, to the House in support or condemnation of either measure.

We have received many communications from correspondents on the subject, all, without exception, approving the provisions contained in the last-mentioned Bill. Some of the writers suggest additions to the "ORDER FOR VACCINATION," and propose that much more information relative to the history and progress of the cases should be entered by the vaccinators. All of these gentlemen, we apprehend, overlook the fact, that the remuneration awarded to the surgeon affords no superfluity for the extra labour that would thus be attached to the operation, and report already demanded in the Order, and none of them suggests a higher payment per case. What right has the framer of the Bill, or the Legislature in passing it, to compel medical practitioners to fulfil commands which neither Parliament nor the rate-payers are prepared adequately to compensate? We are quite disposed to ask the vaccinator to give to the public all the information he has opportunity to communicate on each case; but to require him, under penalty of the whole fee, to amplify his report beyond the simple details already required, and which will certainly accomplish one great object of the Bill, without proposing to pay him for his time and trouble, is unreasonable and unjust. We make these remarks, not in condemnation of the useful suggestions of our correspondents, but simply to account for the absence of any attempt to transfer to the "Order" the various suggestions which it might be desirable to adopt, could pecuniary provision be made for imposing on the vaccinators such extensive duties. However, without making the "Order," unfairly onerous, it shall be offered to the House on the third reading in as effective a shape as is consistent with these views. There is no reason why proposals should not ultimately be made for *voluntary* contributions to the science of vaccination, when the one great object of legislation is accomplished. But already, in every direction, the profession is over-taxed and underpaid for the public advantage.

We shall endeavour to find space in another Number for a summary of the proposals and remarks that we have received. The topic is one of much interest, and the direction of the professional mind, now that it has come before the Legislature, worth attention, on the score both of utility and curiosity.

PETITION FROM GLOUCESTER.

[SIGNED by 12 surgeons and physicians of Gloucester, and forwarded to Mr. WAKLEY, by Mr. H. W. RUMSEY, May 23rd, for presentation to the House of Commons.]

The Humble Petition, &c.,

SHOWETH,—

That your Petitioners, strongly disapproving of the main provisions of a Bill intituled, "An Act to Extend the Practice of Vaccination," which has been passed by the House of Lords, humbly pray that your Honourable House will be pleased to reject the said Bill:

And, further, that your Petitioners having seen a copy of another Bill for the same object, which has been introduced by the Honourable Member for Finsbury, into your Honourable House; and considering the same to be well calculated to promote the benevolent intentions of the Legislature, and to give satisfaction to the medical profession, humbly and earnestly entreat that it may be allowed to pass into a law:

And your petitioners, &c.

Mr. WAKLEY has presented to the House of Commons a Petition from Mr. JOHN HICKS NANKWELL, Surgeon, of Saint Columb, Cornwall, pointing out the grievances of the profession under a system that allows quacks and empirics to profess medicine; that enables the Poor-Law Commissioners to subject them to unjust plans of parochial medical servitude; that gives the members of the Colleges and Halls no self governing power in those institutions; and that places education in one science at the mercy of 19 rival distributors of diplomas and licenses; and praying that these evils may have the opportunity of being abated by the consolidation of the profession into one National Faculty of Medicine.

A PETITION to the House of Commons, against the Lords' Vaccination Bill, has been sent to the Member for Hertford, William Cowper, Esq.

1. *The Nervous System, Anatomical and Physiological, &c.; being the First Volume of an Original System of Physiology, adapted to the Advanced State of Anatomy.* By ALEXANDER WALKER. London: 1834. 8vo, pp. 704.
2. *Documents and Dates of Modern Discoveries in the Nervous System.* London: 1839. 8vo, pp. 172.
3. *Narrative of the Discoveries of Sir Charles Bell in the Nervous System.* By ALEXANDER SHAW. London: 1839. 8vo, pp. 232.
4. *Outlines of Human Physiology.* By HERBERT MAYO. London: 1837.

THE inquiry into the history of certain discoveries in the physiology of the nervous system, which we published in the Number of THE LANCET for April 25, terminated with the labours of Sir Charles Bell. Here the historian might have fairly rested, but the critic has a further and a more delicate duty to perform. It behoves him not only to "render unto Cæsar the things which are Cæsar's," but to detect and expose those who would pilfer the property of others, and clothe their own nakedness in the glory of some friend or master, whom, Judas-like, they have sought to kiss and betray. We have already shown that, although Sir C. Bell did not, in any part of his writings, *distinctly* attribute motion to the anterior columns of the spinal marrow, and sensation to the posterior columns, yet that his facts and reasonings (together with those published by Mr. John Shaw), compel us to infer that such attributes formed part and parcel of his physiological doctrine. Correspondence of function with the roots of a nerve, double function, with double origin, were the key-stones of his theory, and we again repeat, that he was acquainted with the true functions of the spinal marrow, previously to August 1822, although no special announcement of the fact be contained in any one of his memoirs. It was this latter omission, probably, which emboldened Mr. Mayo to set up claims of his own, and attempt the perpetration of one of the vilest literary larcenies that it has ever been our lot to expose. At page 249 of Mr. Mayo's "Elements of Phyiology," we find the following passages:—

"When a spinal nerve is divided, in its course through the body, the parts supplied

by it, beyond the division, are paralysed—they lose sense and motion. If the two origins of the spinal nerves are exposed in a young animal, and separately divided, different effects are produced. The section of the anterior root deprives the part supplied by the nerve of voluntary motion, sense remaining: the section of the posterior root deprives the corresponding part of the body of sensation, voluntary motion being left.

"These experiments were made by M. Magendie, and published by him in his 'Journal of Experimental Physiology.' But many years earlier, Sir C. Bell had made experiments upon the spinal nerves, some account of which had been printed and circulated among his friends, as well as delivered in his lectures. The following is an extract from this account:—'On laying bare the roots of the spinal nerves,' observes Sir C. Bell, 'I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.' Sir C. Bell was carried by these experiments very near to the truth, but he failed, at that time, to ascertain it; he inferred from his experiments, indeed, that the anterior and posterior roots of the spinal nerves have different functions; but in the nature of those functions he was mistaken. Upon the anterior root he supposed both motion and sensation to depend: the posterior root he considered an unconscious nerve, the office of which was to controul the growth and sympathies of parts. Before Sir C. Bell published any other account of the functions of these nerves, M. Magendie had given to the world the true theory of their uses."

Here the discovery is peremptorily awarded to M. Magendie, by Mr. Mayo, for no other purpose, apparently, than that of participating himself in a share of the plunder. But the claims of M. Magendie, if he have any, are easily disposed of. In August, 1822, M. Magendie published an account of some experiments on the spinal nerves (which he affected to consider as being original, although he was, undoubtedly, acquainted with those of Sir C. Bell), and from them concluded, that the functions of the anterior roots are different from those of the posterior; the former appearing to belong more specially to motion, and the latter more particularly to sensibility.* In a second paper, published in October, 1822, M. Magendie gives an account of various other experiments, which he had undertaken subsequently to the publication of his Memoir in August, 1822. This paper is written in a very confused manner; the author hesitates, and does not seem to place much confidence in the accuracy of the conclusions which he draws from his experiments. In one part he says, "These facts, then, confirm those which I had already announced; they seem, however, to prove that sensation does not belong exclusively to the posterior roots, any more than motion to the anterior roots."* Yet soon afterwards we find, "Mr. Bell was very near discovering the functions of the spinal roots; however, he does not seem to have been aware that the anterior roots belong to motion, while the posterior are particularly destined for sensation."

Finally, in a work which M. Magendie has published, conjointly with M. Desmoulins, he distinctly renounces even the theory of isolated properties of the anterior and posterior roots. "The functions of motion and sensation do not, then, belong separately to the two orders of roots."†

Can any one, after having read the above passages, persist in attributing the discovery to M. Magendie? His experiments were a simple repetition of those originally performed by Sir C. Bell, in 1811. His conclusions were doubtful or obscure; and that was necessarily the case, because, as Sir C. Bell had always observed, both in his writings and his lectures, that the question did not admit of being solved by mere experiment on the living animal. But even his own countrymen, so jealous of national honour, have ceased to advocate the claims of M. Magendie. In a late discussion on the physiology of the spinal marrow, at the

* Il me suffit de pouvoir avancer aujourd'ui comme positif, que les racines antérieures et les posterieures des nerfs qui naissent à la moelle epiniere ont des fonctions différentes, que les posterieures paraissent plus particulierement destineés à la sénsibilité tandis que les antérieures semblent plus spécialement liées avec le mouv ment.

* Ces faits sont donc confirmatifs de ceux que j'ai annoncés: seulement ils semblent établir que le sentiment n'est pas exclusivement dans les racines posterieures non plus que le mouvement dans les antérieures.

† L'isolement des deux proprietés dans chacun des ordres de racines n'est donc pas absolu.—*Anat. de Syst. Nerv. p. 777.*

Royal Academy of Medicine, Paris, in which the most distinguished French physiologists were engaged, the discovery of the distinct functions of the anterior and posterior spinal roots was, universally, attributed to Sir C. Bell; not a single speaker thought of connecting with it the name of M. Magendie.

We now turn to Mr. Mayo. The passages in which he puts forward M. Magendie's claims have been already quoted. In a subsequent part of his "Outlines," however, Mr. Mayo, slyly insinuates that he had conjectured (nearly!) the discovery, and was on the point of proving it experimentally, when the result of M. Magendie's researches was published.

"By the experiments and reasoning (says Mr. Mayo, p. 256,) which I have described, I thus established that the ganglionless portion of the fifth and the hard portion of the seventh nerve, are voluntary nerves to parts which receive sentient nerves from the larger or ganglionic portion of the fifth. This happened before the publication of M. Magendie's discovery of the parallel functions of the double roots of the spinal nerves; and, without wishing to assert the least claim to that discovery, I will yet observe, that I was led, by the well-known anatomical analogy between the fifth and spinal nerves, to conjecture, nearly, what M. Magendie proved, and was, indeed, actually engaged in experiments to determine the point, when M. Magendie's were published."

In the above passage, and in those which we have already quoted from Mr. Mayo's "Physiology," are contained the following assertions:—

1. That Sir C. Bell attributed both motion and sensation to the anterior root of the spinal nerves, the posterior root being for the purpose of controlling the growth and sympathy of parts.

2. That before Sir C. Bell published any other account of the functions of the nerves, M. Magendie had given to the world the true theory of their uses.

3. That he (Mr. Mayo) was the first to establish that the ganglionless portion of the fifth and the portio dura were voluntary nerves to parts which received sentient filaments from the fifth.

4. That from the well-known analogy between the fifth and spinal nerves, he (Mr. Mayo) was led nearly to conjecture M. Magendie's discovery.

Here are four propositions of deep interest, and involving questions of great physiological import. The assertions contained in them are, however, each and all, not only unfounded, but contrary to the truth. Let us examine the assertions of Mr. Mayo, briefly and *seriatim*. His web is ingeniously woven, but flimsy withal; it is torn by the slightest touch, and leaves exposed, underneath, a wreck of degrading egotism and detected chicanery.

1. Sir C. Bell never attributed both motion and sensation to the anterior root of the spinal nerves. We have already demonstrated this. The essence of Sir C. Bell's doctrine was, that different functions belonged to different roots and different portions of the brain. In his first proposition, then, Mr. Mayo either has purposely misrepresented the opinions of Sir C. Bell, or been guilty of a most stupid error.

2. Is it a fact, that M. Magendie had given to the world the *true* theory of the spinal nerves before Sir C. Bell had published any other account of their functions? Certainly not. M. Magendie has never published the true theory; and even if his theory were true, he was anticipated by Sir C. Bell. But we beg the reader to mark the crafty, Jesuitical manner in which Mr. Mayo seems to have tried to throw discredit on the labours of his friend and master. Sir C. Bell is represented as entertaining, in 1822, the crude ideas which he put forward as a mere essay in 1811. Although daily, almost hourly, engaged in the study of the nervous system for more than ten years, he is depicted, by Mr. Mayo, as one who made no progress, but blindly adhered to the opinions and errors of his youth. And this absurd assertion is put forth by one who was a familiar pupil of Mr. C. Bell; who had attended his lectures; had assisted in his researches; nay, had been employed to make dissections of the nervous system, demonstrated that very progress which he afterwards affected to deny! Again; the name of Mr. John Shaw is kept in the back ground by Mr. Mayo, and no allusion is made to the experiments or observations of Mr. S.; but it is said, "before Sir C. Bell published any other account," &c. This is an unworthy *suppressio veri*. Before Sir C. Bell had published any distinct account, certainly; but not before the writings of Sir Charles, taken in conjunction with those of Mr. Shaw, had shown that he had

arrived, by experiment, by pathological observations, and by analogical reasoning from the ascertained functions of the fifth and seventh nerves, at a knowledge of the true functions of the spinal nerves.

3. Is it true that, before the publication of M. Magendie's discovery, Mr. Mayo "had established that the ganglionless portion of the fifth, and the hard portion of the seventh nerve (portio dura), are voluntary nerves to parts which receive sentient nerves from the larger or ganglionic portion of the fifth?" Unquestionably not. Upon this point we must enter into a few particulars, which are worthy of being recorded; for the suppression of some facts, and the ingenious distortion of others, by Mr. Mayo, present two of the most remarkable phenomena in the history of modern literature. The reader must recall to mind what we have already noticed, that Sir C. Bell, being unable to determine the functions of the spinal roots by experiments on the spinal marrow, turned his attention to the fifth nerve, which he proved to be analagous to the spinal nerves; and also to the portio dura, which, arising by a single set of roots, served as a contrast to the fifth, or double-rooted cerebral nerve. These facts had been repeatedly pointed out in the writings and lectures of Sir C. Bell and Mr. Shaw; and Mr. Mayo had been employed by Sir C. Bell at the Museum, in Great Windmill-street, in making preparations for the purpose of illustrating the difference in the structure of the roots of the nerves; yet he has the coolness to affirm, that "he established," &c. The name of Sir C. Bell is adroitly connected with points which do not bear upon the question at issue; the name of Mr. John Shaw is totally suppressed. It is not the "Ego et Rex meus" of the aspiring cardinal that Mr. Mayo assumes for his motto. He would supercede his king altogether, and set up himself for worship instead.

But another fact, to which we shall direct the reader's attention, shows how completely undeserving of credit are the assertions of Mr. Mayo, with regard to the history of the researches in question.

At page 256 of his "Outlines of Physiology," Mr. Mayo affirms, that before the month of August, 1822, he had established the true functions of the fifth and seventh nerves, and was led, *from the well-known analogy between the fifth and spinal nerves*, nearly to anticipate M. Magendie's discovery. Now, what are the *facts* of the case? In his "Anatomical and Physiological Commentaries," Mr. Mayo published two memoirs relative to the physiology of the nervous system; the first bears date, *August*, 1822; the second, *July*, 1823. In the first Memoir (which, be it observed, did not precede that of M. Magendie, but was published on the *same* month of the *same* year,) Mr. Mayo makes no allusion whatever to the necessity of investigating the functions of nerves through their roots; he does not notice the original experiment of Sir C. Bell, nor those subsequently performed by Mr. John Shaw; he does not think it worth while to mention, that the fifth nerve arises by two roots, and the portio dura by one root, although he describes, most minutely, the branches of those nerves; he does not allude to the fact of the fifth nerve possessing a ganglion on its posterior root; and, finally, so far from drawing any conclusion from the *well-known analogy* between the fifth and spinal nerves, Mr. Mayo endeavours, in this Memoir, to subvert Sir C. Bell's arrangement of the nervous system, as being "prejudicial to the interests of science, and an unsound theory."

In his second Memoir, published in *July*, 1823, that is, *eleven months after* the publication of M. Magendie's, the whole face of the question changes in the hands of Mr. Mayo. The fifth nerve is now described as one arising by two roots, with a ganglion on the posterior root; its analogy with the spinal nerves is strongly insisted upon, and from analogy of structure is deduced analogy of function. "By this analogy (says Mr. Mayo), I was led to conjecture that the double roots of the spinal nerves have functions corresponding with those of the fifth; and that the large posterior portion of each spinal nerve, with its ganglion, belongs to cutaneous sensation, and the anterior branch to voluntary motion." The reader is, all the time, allowed to conclude, that these important observations are the result of Mr. Mayo's unaided researches; not a syllable is uttered which might call attention to the names of Bell and Shaw.

But this is not all; we beg of the reader again to peruse the extract from Mr. Mayo's "Physiology," which he will find at p. 342, and to bear in mind our analysis of Mr.

Mayo's commentaries. How, then, does Mr. Mayo make it appear that "before the publication of M. Magendie's discovery of the parallel functions of the double roots of the spinal nerves, he (Mr. Mayo), was led by the well-known analogy between the fifth and spinal nerves," &c. &c.? Simply, by suppressing the dates of his commentaries, by confounding their contents together, and by making it appear that the Memoir of *July*, 1823 (which contained the true doctrine), and that of *August*, 1822 (which was written for the purpose of overthrowing it), formed, together, a whole or continuous work, published before the month of August, 1822.

Upon this thimble-rigging manœuvre rest the scientific claims of Mr. Herbert Mayo. We shall not stoop to criticise it. In the mind of every honest man it will remain graven as a proceeding that is surpassed in character and quality only by that which has latterly placed in juxta-position the names of Home and Hunter.

MEDICAL SOCIETY OF LONDON.

Monday, May 25, 1840.

Dr. Clutterbuck, President.

MEDDLESOME MIDWIFERY.

Dr. Severn thought the case related last week was one of such extreme rarity, that its occurrence would offer no valid objection to the employment of instruments in those cases of *dead* children, in which such a proceeding was deemed necessary, and in which the evidence of death rested mainly upon the exfoliation of the cuticle.

Mr. Proctor differed from the last speaker. He could not agree that because a case like that under discussion was rare, the destruction of the vitality of the child should, therefore, be determined on in cases in which it was thought necessary to use instruments. He thought, as a general rule, instruments in midwifery practice, provided the child were in the proper position, and no hæmorrhage was present, should not be employed. When the patient had confidence in her attendant it mattered little how long she was in labour. There were intervals of rest during its progress, and he knew of no law which prescribed a certain time in which it was necessary for a labour to be terminated. In cases in which no urgent symptoms required interference, labour might safely go on for three days.

Dr. Chowne defended the use of instruments, when applied at a proper period, under proper circumstances, and by a competent practitioner. With regard to the case he had related at the last meeting, he might observe, that Dr. Blundell had alluded to the rare occurrence of such cases, in his lectures. He (Dr. Chowne) had omitted to mention that the abdomen of the child was in the same condition, with reference to the cuticle, as the other parts of the body.

Mr. Dendy believed all the instruments employed in midwifery, except the long forceps, might be safely used in proper hands. There were cases in which instruments were absolutely necessary, and he had never had occasion to regret their employment when he had been called upon by circumstances to use them. He thought, in a case like that under discussion, instruments would be rarely necessary, as the child would be so dwindled that it would pass readily through the passages.

Mr. Leese thought the judicious employment of instruments often saved much suffering to the mother, and preserved the life of the child. If employed more frequently, provided proper judgment were exercised, he thought benefit would result. Many cases in which ergot of rye was administered, might be more advantageously terminated by the use of the forceps.

Several members now bore testimony to the specific action of the ergot of rye on the gravid uterus, in consequence of the President expressing a doubt whether it had any such influence. Most of the speakers considered, that this medicine was at present much more frequently used than was advisable.

Mr. Pilcher related a case, in which rupture of the uterus resulted from the pressure of the knuckles of the accoucheur on the walls of the uterus during the operation of turning.

This being the last meeting of the Society for the session, the President made a short address on leaving the chair.

MICROSCOPICAL SOCIETY OF LONDON.

Wednesday, May 20, 1840.

Richard Owen, Esq., President.

The Secretary read the Minutes of the last Meeting, which embraced an abstract of a paper communicated by Dr. Lindley, upon the Antheridiæ of Sphagnum. The antheridiæ are made up of cells which contain a mucous substance, and within each of the cells a vibrio is coiled round.

Mr. Bowerbank had shown to the Society four new forms of vegetable cellular tissue, in specimens of fossil wood from Antigua.

Mr. Dalrymple then read a paper upon the

FAMILY OF THE CLOSTERINÆ.

After discussing the early opinions and researches upon these microscopic beings, Mr. Dalrymple described them to be either of a crescentic figure or spindle-shaped; the exterior is a siliceous shell, divided transversely in the centre, and embossed with longitudinal lines. In their interior is a green gelatinous and granular matter, presenting a series of spheroidal cavities, connected in one species by a continuous canal. Within these cavities were thousands of moving, active particles. At each extremity is a transparent chamber, in which are a number of transparent moving spherules, about the 30,000 of an inch in diameter. Similar bodies move backwards and forwards slowly and vaguely, between the case and the green matter, from one end of the being to the other. This circulation is, however, very irregular, and the molecules are too minute to ascertain whether they possess cilia. Mr. Dalrymple's observations disproved the existence of the papillary locomotive organs, described by Ehrenberg, as existing upon the transparent chambers at the extremities of closterium. There are no openings in the transparent chamber at the extremities of the shell. The movements of closterinæ are slow, but sometimes exhibit a slight vivacity; they adhere to solid bodies by their extremities, as if by means of a sucking apparatus. The transparent chamber is probably a respiratory apparatus, driving out the water which it contains by its contractions, and filling again by endosmose. Such an arrangement may not be needed in the polygastric infusoria covered over every part of their surface with cilia; but in closterinæ contained within a siliceous shell, closed by membrane at each extremity, and living in still water without active powers of motion of its own, the fluids, without some special apparatus, would be very imperfectly aerated.

Ehrenberg places the closteria among the polygastric infusoria, for four reasons:—1. Because they possess voluntary motion; 2. They have an opening at each extremity; 3. They have a moveable locomotive organ at each extremity; 4. They have a transverse spontaneous division. These four characters separate closterium from all known plants, and rank them, naturally, with infusoria; for all supposed plants that have voluntary motion, open mouths, foot, and transverse spontaneous separation, one would, without fear of reproach, class with animals, even without absolutely seeing them eat.

In reply to Ehrenberg's opinion, Mr. Dalrymple observes: — 1. Many plants have motion, and it is difficult to say whether the vague and slow movement of closterium be voluntary or not. 2. The openings at either end are doubtful; in some they certainly do not exist; in others, Meyen states the appearance to be a mere indentation. 3. The moving organ has no existence; and the moving molecules are not dissimilar to those seen in the cells of some plants, as the jointed confervæ. Lastly, transverse division holds with all jointed confervæ.

Meyen in trying chemically whether closterium was animal or vegetable, states that the green matter became violet purple on the addition of iodine. This would have been satisfactory with regard to the amylaceous nature of the contents of the shell; but Mr. Dalrymple, on repeating this experiment, does not find any blue colour produced. When some of the green contents were squeezed out, they were eagerly devoured by the coleps hirtus, a well-known animal feeder amongst the infusoria.

The modes of reproduction in this family are three:—1, by spontaneous transverse division; 2, by ova, or sporules; 3, by a process of interbudding, in which the green matter of two closteria leaves the shells at the transverse constriction, and forms a rhomboidal mass. This is said to resemble the mode of propagation in confervæ; certainly it is wholly unlike animal reproduction.

Mr. Dalrymple is disposed to retain closterinæ on the side of the animal kingdom, for the following reasons:—1. Because, although they possess a circulation of molecules like plants, they have a definite organ, in which the molecules appear to enjoy an independent motion, and the parietes of which appear capable of contracting upon their contents. 2. The green matter is contained in a membranous envelope, which contracts upon the application of certain non-chemical stimuli; tincture of iodine, in extremely small quantity, produces speedy death and a singular and violent contraction of this membranous envelope, by which it is eventually burst. 3. The supposed ova contain a vitelline nucleus, a transparent molecular fluid, and a chorion, and are, therefore, dissimilar to the cytoblasts of vegetables. 4. The existence of a suctorial apparatus at each extremity. 5. The absence of the blue colour in the green substance on the application of iodine.

Mr. Dalrymple's paper was illustrated with numerous beautiful water-colour drawings.

Mr. Cornelius Varley made some observations on the structure and habits of the closterinæ, which tended, in his opinion, to establish their vegetable nature.

After a few remarks from the President, the Society resolved itself into a conversazioné, for the purpose of examining the closterinæ, and other interesting specimens

from the pigmy world, with the aid of the microscope.

*** From mismanagement in the disposition of the microscopes, crowded as they were upon a small table, instead of being placed in convenient situations, upon the large and well-adapted table which occupies the centre of the room, we were unable to obtain a peep at any of the preparations exhibited. We only express the general feeling of the Fellows of the Society in calling the attention of the Council to this point, and are satisfied, from our knowledge of the intentions of that body, that we shall not appeal to them in vain.—Sub. Ed. L.

ANATOMICAL AND PHYSIOLOGICAL SOCIETY OF EDINBURGH.

At a meeting of this Society, held in the Medical School, Argyle-square, on Wednesday, 1st April, the following communications from the pen of Dr. Knox, of that city, were read, and the preparations illustrative of the structures exhibited.

I. ON THE SUPPOSED ELASTIC TISSUE OF THE HEART.

Notwithstanding the numerous researches, as well Anatomical as Physiological, which have been made into the structure of this important organ from very remote times, and with more or less success, a new candidate has appeared claiming the honour of having discovered an additional structure in the composition of the heart, and that, too, a very important one, provided his views be fully adopted by the Physiologist,—I allude to M. Deschamp's, whose Memoir was read, very lately, to the Royal Academy of Medicine in Paris. This Memoir, which I have as yet but very cursorily examined, states, amongst other matters of less consequence, and more generally admitted, that the heart is composed of four distinct layers, viz., 1st, a serous, derived from the serous portion of the pericardium; 2nd, a muscular layer; 3rd, *an elastic layer continuous with the elastic or fibrous tissue of the arteries and veins;* and 4th, a membrane, being the inner investing membrane of the heart, also continuous with the inner tunic of the arteries and veins.

It is difficult to say how far this discovery of M. Deschamp will be admitted to be both new and true by anatomists and physiologists; but, leaving the task of criticism to others, I shall take the liberty here of directing the attention of the Society to some views, which I have long entertained, respecting the nature of the tissues which lie on the external side of serous membranes, and to which membranes the inner tunic of the heart and great vessels has been very generally admitted to bear some slight analogy. If the free surface of a serous membrane be slightly scratched with a knife, and then stripped off with the forceps, it will be found to leave behind it a stronger tissue, intimately united by cellular texture to the adjacent organs; but my attention was more especially directed to the nature of these two outer textures, by what we observe when a tube, or duct, such as the ureter or vas deferens, approaches a serous membrane closely, into the textures of which, as it were, they penetrate, thus acquiring a sheath at the expense of the outer portions of the more external parts of the serous membrane itself, and of the adjoining cellular tissue; in short, every anatomist knows that these ducts, or tubes, leave between their walls and the serous cavity, merely the thinest layer of that membrane which we usually term serous. I next observed, in examining the anatomy of the arachnoid membrane (the principal results of which examination were published by me in an early Number of this winter's Lancet), that the structure lying between what we usually term arachnoid and pia mater, were altogether peculiar; and that the connection between the outer surface of the arachnoid and the pia mater does take place, not merely by means of common cellular tissue, but that the intermediate space was filled, and the connection established, by means of numerous, glistening, distinctly tendinous fibres, passing from the one membrane to the other, and of whose tendinous and fibrous character there could not be a doubt. So soon, therefore, as I observed a notice of M. Deschamp's paper, it occurred to me that his elastic tissue might be viewed more as an appendage to, or an integral part of, the general character of serous membranes (and of membranes analogous to serous) all over the body; and from a hasty examination made of the heart, during the course of the morning, I feel disposed to maintain this view in opposition to that of M. Deschamp, who thinks the structure in question a continuation of the peculiar arterial and venous tissue, composing the middle tunic of the artery and *proper* tunic of the vein.

If the innermost membrane of a heart, for example, which has been kept for a day or two, be examined by laying open the pulmonary artery and right ventricle, the following structure, upon a very superficial examination, may be easily made out.

Select one of the larger *columnæ carneæ*, connected with the free edge of the tricuspid valve by tendinous cords, and cautiously cutting through the inner membrane as it covers the fleshy column, it will be found that another and a denser tunic lies exterior to it, and that the chordæ tendineæ are appendages of this outer membrane. So far this dissection is not opposed to M.

Deschamp's views; but if we examine that part of the ventricle in immediate connection with the artery, we shall find that the structural arrangement *is* opposed to these views, as is demonstrable by the following dissection:—Raise up the inner membrane of the heart from off the ventricle, adjoining the semilunar valves, and draw it towards these valves, of which it forms a principal part; then remove the elastic tissue exterior to this delicate membrane, and separating it from the fleshy fibre of the heart (with which it has the most intimate union), and drawing it towards the valves also, into which it enters, it will be found that it does not go on to constitute the middle tunic of the artery, but that tunic (*middle*) stops immediately at the cartilaginous circle from which, in this part, the muscular fibres of the heart arise, and by means of which the artery is, in the most intimate manner, united to the heart itself. Although, therefore, this view be opposed to that of M. Deschamp, it does not in any way detract from his merit in having called the attention of the physiologists to this structure.

I shall take the liberty of addressing the Society, on a future occasion, with regard to the physiological consequences which flow from the presence of such a structure in the heart, confining myself for the present to a single remark. A few years ago two French physicians, Messrs. Leuret and Lessaigne, published a memoir on the human pulse, in which they asserted, contrary to the experiments I had made in 1812 and 1813, that the pulse increases in *rapidity* with age, and that the evening pulse of every person accelerates and exceeds that of the morning. So soon as I observed their memoir, I returned to the subject of the pulse, and repeating the whole series of observations upon an extended scale, published the results in the "Medical Gazette" of 1836. The conclusion I came to then, was that the French observers were entirely in error, the causes of which, from being unacquainted with the *locality* of their observations, I could not well point out, but I felt assured of their being in error; and a complete confirmation of the correctness of my views, with regard to the statistics of the human pulse, was soon after published by Dr. Guy, of Cambridge. It gives me a good deal of satisfaction, however, to find, from M. Deschamp's paper, that he had repeated the observations of Messrs. Leuret and Lessaigne *in the same public institution* which furnished them the means of their original remarks, and has proved them, as I originally stated, to be incorrect.

To return for a moment to the anatomical part of the memoir of M. Deschamps, I may take the liberty of repeating, shortly, what has been done more at length in the preceding remarks. The *elastic tissue*, found on the outer side of the innermost membrane of the heart, the chordæ tendineæ of the ventricles, and other tendinous structures which more or less enter into the composition of the valves, I conceive to be parts of one system of structures, analogous to those which we find on the outer side of serous membranes, in other parts of the body, *and more especially* of the arachnoid.

R. KNOX.

II. OBSERVATIONS ON THE PATHOLOGY OF PUERPERAL FEVER.

It has been known for some time, that in the South of Scotland and North of England, a puerperal fever of a very fatal character prevails in several districts, causing a good deal of alarm to those who are practising the obstetric art. Already a considerable number of fatal cases has occurred; and as, for many reasons, it was desirable that every possible inquiry should be made into the cause of so dangerous and fatal a malady, I was requested, by a distinguished practitioner here, to give an opinion respecting the morbid appearances found in two fatal cases. In stating the result I shall be as brief as possible, being sensible how much has been done on the subject of the pathology of puerperal fever since the time of Mr. Hunter, who, if I may judge from a memorandum taken from a MS. copy of M. Hunter's "Lectures on Surgery" (and which I accidentally met with many years ago), viewed the cause of puerperal fever to be merely peritoneal inflammation. There can be no doubt that in one respect Mr. Hunter was right, and that the sero-purulent effusions which are so generally found in the cavity of the peritoneum, after death from puerperal fever, and which were also found in the cases which form the subject of this paper, can have their origin solely in inflammation of a serous membrane. Whether or not this was the whole amount of Mr. Hunter's knowledge we cannot now determine, so far as I know.

I have myself examined the morbid appearances in about thirty fatal cases of puerperal fever which have occurred in Edinburgh, or elsewhere, having been frequently requested to do so by accoucheurs, who declined handling the abdominal viscera in such cases from a dread of the morbid fluids possessing contagious properties. In nearly all these cases a sero-purulent fluid was constantly present in the peritoneal cavity, and not unfrequently obvious marks of peritoneal inflammation extending to its serous surface; but every pathologist knows that in addition to these appearances, which are not absolutely constant, there are others of a much more decided and extended character, altogether exterior to the serous membrane, properly so

called, consisting in purulent deposits more or less extensive in the subserous cellular membrane; veins filled with purulent fluids, and whose walls seem, also, to have partaken in the inflammation; and *lymphatic vessels*, which are found filled with well-digested pus, even admitting that the walls of these vessels may not have partaken of the general inflammation of the surrounding parts. In addition to this, inflammation, with softening of the ovaria, is also frequently present. It must not be supposed, however, that all these appearances are found in every case; more generally, I think, the seat of mischief seems to be in the veins, which, together with the surrounding sub-serous cellular tissue, are filled and infiltrated with purulent fluids. On the other hand, but more rarely, the lymphatics alone seem to be the seat of the disease, and it is sufficiently remarkable, that in both the preparations which form the subject of this paper, the veins seem healthy, whilst the *lymphatics* were gorged with pus. This, undoubtedly, is the rarer form of the disease; although also sufficiently frequent.

Of late years, a new field of inquiry has been opened up in respect to the presence of pus globules in the blood, derived from such sources as that we now speak of; and although it can scarcely be doubted, that such will be found in the general mass of the blood in puerperal cases, seeing that it exists in such quantities in the veins and lymphatics, it were yet extremely desirable to put the matter beyond a doubt by the microscopic examination of the blood itself; such an inquiry, independent of the verification of a useful physical truth, must, one would imagine, ultimately contribute to throw new light on the treatment of a disease, notoriously beyond the power of medicine in general, and respecting the treatment of which, so far as I have been able to observe, the opinions of practitioners are altogether unsettled.—R. Knox.

BRITISH MEDICAL ASSOCIATION.

Exeter Hall, May 19, 1840.

Dr. Marshall Hall, V.P., in the Chair.

The Minutes of the last meeting were read and confirmed.

It was stated by the Chairman, that a Deputation of the Council, consisting of Dr. Webster, Mr. Davidson, and Dr. M. Hall, had waited upon Mr. Warburton, M.P., on the subject of Medical Reform, and his intentions thereon, and that the result of the interview was gratifying and satisfactory to the Deputation. A Report of the interview was ordered to be presented at the next Meeting of Council.

A letter was read from Dr. Ross Lietch, of Tynemouth, on Medical Remuneration in Poor-Law Unions; and in reply it was recommended that Petitions should be presented from all Union Surgeons, praying that the Scale of the Parliamentary Medical Witnesses, and the Minute of the Poor-Law Commissioners, should be introduced into the new Bill and enforced.

A letter was read from Mr. Carter, of Newcastle-upon-Tyne, on Vaccination; a second letter was read from the same gentleman on Medical Reform, which was referred to the Reform Committee.

A letter was read from H. C. Attenburrow, Esq., Secretary to the Nottingham Branch of this Association, communicating the result of an interview with one of the County Members, who considered the Presentation of Petitions for Medical Reform as entirely useless at the present moment, there being no question on the subject before the House of Commons, and recommending that Petitions should be reserved till some motion was before the House. Under these circumstances Mr. Attenburrow applied for information. An answer had been sent to the effect, that as *many Petitions as possible* should be immediately obtained, and *forthwith presented, previous to the question coming formally before the House*. 1st, That Honourable Members may be duly apprised of the wishes and wants of the great body of the Profession. 2ndly, To show that nothing but a complete searching measure of Medical Reform will give satisfaction; and, 3rdly, That the monopolists may not be able to say no Petitions have been presented! No reform is wanted!

A letter was read from J. Livingstone, Esq., Secretary to the Eastern Medical Association of Scotland, requesting information relative to the plan on which the Benevolent Fund of the British Medical Association is founded, as a similar fund has been suggested to be attached to the former.

Drs. Hurst and Marshall were introduced to the meeting as a Deputation from upwards of 500 Medical Practitioners (Physicians and Surgeons) in the province of Ulster, Ireland, and laid before the meeting a statement of grievances connected with certain proceedings of the Company of Apothecaries in Dublin.

The substance of their remarks is contained in the following document:—

We, and our brethren, having served a short apprenticeship to the compounding of medicines, afterwards complied with the extensive requirements of the Universities and Colleges of Surgeons, in London, Dublin, Edinburgh, and Glasgow, and received our degrees in medicine, and diplomas in surgery therefrom.

With these qualifications we have been practising, in every branch of the profession (many of us for twenty years and upwards), undisturbed, and enjoying the public approbation, as the Petitions to be shortly presented to Parliament will show.

In 1791, a Company, of *mere apothecaries*, in Dublin, obtained an Act of Parliament, "To Restrain and Punish *Ignorant and Unskilful Persons*," who might practice as apothecaries, without a licence from the said Company. This Act was not generally enforced until the year 1827; consequently, we were either not aware of its existence, or believed it to be a dead letter, which, in any event, *could never apply to regularly educated physicians and surgeons*, such as we are.

However, from 1827 till 1829, the Apothecaries' Hall, as appears by a Parliamentary Return, realised £5000 by *fines* exacted from the physicians and surgeons who practised as general medical Practitioners in Ireland, and even *gave their license to great numbers of notoriously ignorant persons on their paying the penalties.*

In this course they were arrested by a remonstrance from Lord Stanley, when Chief Secretary for Ireland.

They have now taken legal proceedings against us, and our brethren, with the view of recovering from each the sum of £40 as penalties, and, if they succeed, will realise a sum of £20,000.

As the Hall refuses us any examination, to prove our competency in pharmacy, *there is no remedy, except by Government interference, or a short Act of Parliament, to stay proceedings, and to be protective of what we consider our legal rights and privileges.*

A precedent for such interference may be found in an Act passed in the present Session of Parliament, protective of, and to stay legal proceedings against, persons entering in their own names more than one horse on a race-course, contrary to a former Act of the Legislature on this subject. Surely, then, such interference and protection will not be denied to us.

The meeting listened with attention to the statements of the Deputation, and promised to assist them by every means consistent with the rules and objects of the Association.

Resolved unanimously, "That Drs. Hurst and Marshall be elected Corresponding Members of the British Medical Association.

The Meeting then adjourned to June 2nd.

ANNOUNCEMENT.

To the Editor of THE LANCET.

SIR:—I am directed to request your insertion of the enclosed in your Journal, as an article of general information. I am, Sir, your most obedient servant,

EDMUND BELFOUR, Sec.

College of Surgeons, May 26, 1840.

ROYAL COLLEGE OF SURGEONS IN LONDON.

The President and Council have great pleasure in announcing to their members, that three studentships in human and comparative anatomy have been instituted by the College, to be held respectively for the term of three years, with the annual stipend of one hundred pounds attached to each studentship, and that, at the instance of the Director-General of the Medical Department of the Army, the Physician-General of the Royal Navy, and of the Chairman of the Honourable East India Company, the General Commanding the Army in Chief, the Lords Commissioners of the Admiralty, and the Court of Directors, have, with the view of promoting the objects of the College, been pleased to place at the disposal of the President and Council, an assistant surgeoncy in each service, once in three years, for such of the said students as may be considered worthy of these honourable distinctions.

The President and Council have also the pleasure to announce that, with the view of rendering the prizes granted by the College more worthy of competition amongst their younger members, they have augmented the Collegial (triennial) Anatomical Prize from thirty to fifty guineas; and have added ten guineas to the like sum, allotted by its founder, to the Jacksonian (annual) Surgical Prize. (By order)

EDMUND BELFOUR, Sec.

May 18, 1840.

*** These announcements ought to be handed to the publisher, and paid for as advertisements.

DR. CAPE AND HIS PUPILS.

AT a meeting of the Students of St. Thomas's Hospital, held May 19th, it was unanimously resolved, That "a letter be addressed to Dr. Cape, expressing the great dissatisfaction entertained by this meeting with regard to certain articles, calculated to prejudice his character as a teacher, which have appeared in THE LANCET, and other periodicals of last week; and to assure him of their continued confidence, respect, and esteem." (Signed)

CHARLES WELLINGTON KENT, Chairman, and 40 of the Students.

In conformity with the above resolutions, the following letter was sent to Dr. Cape:—

SIR:—I have the honour to transmit to you the resolutions unanimously agreed upon at a Meeting of the Pupils of St. Thomas's Hospital, on Tuesday last, and respectfully to express to you their high approval of the line of conduct which you thought it your duty to pursue, during the *contention* to which these resolutions refer; which course they feel convinced tended both to the honour of the school and the dignity of the Chair of Midwifery. I beg

to assure you, that so far from their confidence and esteem being in the least diminished, the manner in which you have acted in this affair has greatly contributed to increase both.

I remain, Sir, yours respectfully,

CHARLES WELLINGTON KENT.

1, Duke-street, London Bridge,
May 20, 1840.

*** "Save us from our friends" is an aspiration which Dr. Cape will, doubtless, fervently ejaculate on reading the above letter. A "contention" is admitted by forty-one of his warmest supporters; a contention between a lecturer and his pupils, as to the justice of the award of an insignificant prize. The truth of the statement contained in our Journal of May 16, is not, for one instant, impugned; but the gentlemen,—and while we applaud their kind intentions, we regret the existence of a cause for their exercise,—assembled at an especial meeting for considering the matter, agree to address a letter of condolence to their teacher, expressing their great dissatisfaction—not at the occurrence of such indelicate proceedings, as those which gave rise to our correspondent's censure, within their own walls,—but at the appearance of an account of those proceedings in THE LANCET; and this they go on to state for the purpose of upholding the "honour of the school and the *dignity* of the Chair of Midwifery." Better, far, had the dignity of the occupant of that chair never been placed in the position to require such expressions and assurances; but, unfortunately, these same assurances are not the expressions of their opinion, that their teacher is innocent of the charge which has been brought against him by our correspondent, but simply that he has comported himself valiantly in the conflict; that "the line of conduct which he thought it his duty to pursue, the manner in which he has acted in this affair (of honour?) has greatly contributed to increase their confidence and esteem."—SUB. ED. *L.*

DEAFNESS OF LORD W. RUSSELL.

To the Editor of THE LANCET.

SIR:—I am led to notice the communication of your correspondent J. H., relative to the deafness of Lord William Russell, which appeared in your last Number, not from any apprehension that it will be successful in raising prejudice against the admitted valuable acquisition of catheterism of the Eustachian tubes to our therapeutics in deafness, but from a desire to adduce his Lordship's case as exemplifying, in a remarkable manner, the greater suitableness and advantage of the flexible silver catheter over the flexible gum-elastic catheter adopted by Deleau.

Deleau was the French surgeon alluded to by your correspondent; and in narrating the circumstance to me, his Lordship, certainly, spoke of the extreme pain the instrument caused him, when attempted to be introduced by that practitioner; but I do not recollect his speaking of any *subsequent* suffering. Deleau made two unsuccessful attempts, and then relinquished the case.

In the months of April and May of the last year, Lord Wm. Russell was my patient. After a careful inquiry into the history and progress of the case alluded to, I came to the conclusion that it originated in a morbid affectation of the mucous membrane, of which there was *primâ facia* evidence in the difficulty under which his Lordship laboured in breathing readily through the nose; in the appearance of irritability, even of the cuticular lining of the external meatus, and in the chronic inflammatory appearance of the mucous membrane of the fauces.

From the same cause, each meatus narium had become so contracted that I could not introduce my smallest sized catheter. I was, therefore, under the necessity of having one made expressly; and this I succeeded in introducing *without the slightest pain or inconvenience* to my patient. In the course of a few days, after having operated with the same catheter several times, I succeeded in introducing a larger catheter, and, again, a still larger, and also in introducing mild astringent injections, through the catheter, into the tympanic cavities.

I regret that these measures, which appeared to me to afford the only chance of amendment, failed, and, after *fifteen or twenty operations*, I did not consider it advisable to *persevere* longer. At the same time, I may congratulate myself on having added somewhat to his Lordship's personal comfort, by enabling him to breathe more readily through the nostrils.

Deleau's adherence to the gum-elastic catheter, in the face of advantages possessed by the other, is a cause of surprise with all who are acquainted with the subject. I am, Sir, your very obedient servant,

JAMES YEARSLEY.

29, Sackville-street, May 26, 1840.

TO CORRESPONDENTS.

By a letter from Stourbridge we are informed, that a cause was to come on for hearing before the Master of the Rolls, entitled "Betts v. Dudley," on Wednesday last, involving questions of the greatest importance to the medical profession. It must occur too late for notice in our columns this week, but, if the above information be correct, it shall receive attention in the next Number.

ERRATUM.—The case of "Extensive Mortification" in our last Number, is by Henry P. Finucane, of Darley-street, Bradford.